DOCUMENT RESUME

ED 338 923 CE 059 749

AUTHOR	Cave, George; Doolittle, Fred
TITLE	Assessing JOBSTART: Interim Impacts of a Program for School Dropouts.
INSTITUTION	Manpower Demonstration Research Corp., New York, N.Y.
SPONS AGENCY	EXXON Corp., New York, N.Y.
PUB DATE	Oct 91
NOTE	336p.
PUB TYPE	Reports - Evaluative/Feasibility (142)
EDRS PRICE	MF01/PC14 Plus Postage.
DESCRIPTORS	Adolescents; Basic Skills; *Dropout Programs; *Educational Attainment; *Employment Level; Employment Programs; Federal Programs; Females; Followup Studies; High School Equivalency Programs; Job Placement; Job Training; Males; Outcomes of Education; *Program Effectiveness; Program Implementation; Secondary Education; Success; Unemployment; Welfare Recipients; Welfare Services; Youth Programs

ABSTRACT

JOBSTART, a federally funded demonstration program, offered basic education, occupational skills training, support services, and job placement assistance to young school dropouts. A study of the impacts of JOBSTART was designed to separate effects of the program from events attributable to other factors. A total of 2,312 people eligible for JOBSTART were randomly assigned: 1,163 to the experimental group and 1,149 to the control group. Thirteen sites conducted random assignments over varying periods of time from August 1985 to November 1987. Follow-up surveys at 12 and 24 months after assignment gathered data on outcomes such as participation in education and training programs, educational attainment, employment, earnings, and use of public benefit programs. An examination of JOBSTART services and participation at the 13 sites found great diversity within the general framework of the guidelines; longer and more substantial participation than that in other programs for young school dropouts; and similar participation hours among different groups and types of sites. JOBSTART had strong impacts on educational attainment--completion of high school or receipt of a General Educational Development certificate. The impacts on employment and earnings were encouraging for mothers; small, but slightly positive, for other women; and generally negative for men. JOBSTART was sometimes effective and sometimes ineffective in yielding second-year labor market gains in brokered programs and in-house programs; it also showed varying success in both concurrent programs and sequential programs. (An executive summary, 64 tables, and 10 figures are provided. Appendixes include additional data tables and 55 references.) (YLB)

****

* Reproductions supplied by EDRS are the best that can be made *
* from the original document. *

****

CE

ED338923

ASSESSING JOBSTART

Interim Impacts of a Program for School Dropouts

George Cave
Fred Doolittle

October 1991

Manpower Demonstration
Research Corporation

CE059749

U.S. DEPARTMENT OF EDUCATION
Office of Educational Research and Improvement
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

☑ This document has been reproduced as received from the person or organization originating it

☐ Minor changes have been made to improve reproduction quality

● Points of view or opinions stated in this document do not necessarily represent official OERI position or policy

"PERMISSION TO REPRODUCE THIS MATERIAL HAS BEEN GRANTED BY

TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)."

MDRC

BEST COPY AVAILABLE

BOARD OF DIRECTORS

RICHARD P. NATHAN, *Chairman*
Provost, Rockefeller College
State University of New York
Director, Rockefeller Institute
of Government

BERNARD E. ANDERSON, *Vice Chairman*
President
The Anderson Group

PAUL H. O'NEILL, *Treasurer*
Chairman and CEO
Alcoa

ELI GINZBERG, *Chairman Emeritus*
Director
Conservation of Human Resources
Columbia University

MARY JO BANE
Professor of Public Policy
John F. Kennedy School of Government
Harvard University

RAMON C. CORTINES
Superintendent
San Francisco Unified School District

ALAN KISTLER
President
Human Resources Development Institute
AFL-CIO

RUDOLPH G. PENNER
Senior Fellow
The Urban Institute

ISABEL V. SAWHILL
Co-Director
Changing Domestic Priorities Project
The Urban Institute

ROBERT SOLOW
Institute Professor
Massachusetts Institute of Technology

GILBERT STEINER
Senior Fellow
Brookings Institution

MITCHELL SVIRIDOFF
Senior Fellow
Community Development Research Center
New School for Social Research

WILLIAM S. WOODSIDE
Chairman, Sky Chefs, Inc.
Former Chairman and CEO,
Primerica Corporation

JUDITH M. GUERON
President
Manpower Demonstration Research Corporation

MDRC

ASSESSING JOBSTART:
INTERIM IMPACTS OF A PROGRAM FOR SCHOOL DROPOUTS

George Cave
Fred Doolittle

Manpower Demonstration
Research Corporation

October 1991

Funders of the JOBSTART Demonstration:

U.S. Department of Labor
The Rockefeller Foundation
The Ford Foundation
Charles Stewart Mott Foundation
The William and Flora Hewlett Foundation
National Commission for Employment Policy
AT&T Foundation
Exxon Corporation
ARCO Foundation
Ætna Foundation
The Chase Manhattan Bank
Stuart Foundations

Dissemination of MDRC reports is also supported by our Public Policy Outreach funders:

The Ford Foundation
The Ambrose Monell Foundation
Alcoa Foundation
Exxon Corporation
Philip Morris Companies
Ætna Foundation
Metropolitan Life Foundation
The Bristol-Myers Squibb Foundation
BellSouth Corporation

The production of this report was made possible by a grant from Exxon Corporation.

The findings and conclusions stated in this report do not necessarily represent the official positions or policies of the funders.

Library of Congress Cataloging-in-Publication Data

Cave, George.
Assessing JOBSTART: interim impacts of a program for school dropouts/
George Cave, Fred Doolittle.
p. cm.
Includes bibliographical references.
1. High school dropouts–Employment–United States.
2. Occupational training–United States. I. Doolittle, Fred C.
II. Manpower Demonstration Research Corporation. III. Title.
HD6273.C39 1991
331.3'42592'0973–dc20 91-16482
CIP

Copyright © 1991 by the Manpower Demonstration Research Corporation

ACKNOWLEDGMENTS

This report is the product of the efforts of many people at the demonstration sites and MDRC and on MDRC's Board of Directors and its JOBSTART Advisory Committee. Those involved brought research and program operation experience to the project.

Our first debt is to the staff at the demonstration sites, especially the JOBSTART counselor/coordinators. Their efforts turned the idea of JOBSTART into a reality for young people in their communities.

At MDRC, many people played a crucial role in preparing this report. Robert Ivry has been active from the very beginning of the project, helping to conceive the program model, raising funds for the demonstration, and reviewing all reports. Gordon Berlin, Barbara Goldman, Judith Gueron, Milton Little, and Edward Pauly reviewed and provided comments on earlier drafts of this report. Marilyn Price and Sharon Rowser, members of MDRC's Operations Department, helped prepare ratings of JOBSTART's implementation at the various sites.

Processing the amount of data needed for a report of this type is a major effort, involving the work of many people at MDRC. In the Information Services Department, Karen Paget coordinated data collection and systems development for this phase of the project; Darlene Hasselbring supervised the survey effort (by Abt Associates Inc.); and Anita Kraus did the computer programming. The data on program costs were collected by a team led by James Riccio, which included Barbara Fink, Johanna Walter, and Lorien Rice. Bridget Dixon and Shera Ahmad produced and helped analyze the implementation and impact data on which the findings of this report are based. Christine Vincent and Leah Curtin produced the graphics for the report. Judith Greissman edited the report with the assistance of Suzanne Wagner and Sylvia Newman; Patt Pontevolpe, Claudette Edwards, and Stephanie Cowell typed the numerous drafts of manuscripts and tables.

The JOBSTART Demonstration was guided by members of MDRC's Board of Directors and its JOBSTART Advisory Committee, chaired by Bernard Anderson. For their thoughtful comments on an earlier draft, the authors would especially like to thank Bernard Anderson, Eli Ginzberg, and Alan Kistler from our Board; Burt Barnow, Richard Elmore, Robinson Hollister, and Margaret Simms, who are members of the Advisory Committee; and Orley Ashenfelter and Andy Sum, who served as outside reviewers.

The Authors

PREFACE

One of the major challenges on the domestic policy agenda is to improve the skills levels and employment prospects of disadvantaged young people. For those without a post-secondary education, the labor market situation continues its downward spiral. For youths who have dropped out of high school, the problem is particularly acute. While many ideas have been put forth for alleviating this problem – most recently in a rash of reports on school reform and labor market trends – there is no compelling evidence about what works.

The underlying hypothesis of the JOBSTART Demonstration was that a comprehensive program incorporating many of the key features of the widely respected, primarily residential Job Corps – basic education, occupational skills training, support services, and job placement assistance – could succeed for high school dropouts in nonresidential settings. Success was to be defined as increases in educational attainment, employment, and earnings. Thirteen organizations were selected to implement JOBSTART and to cooperate with the multifaceted evaluation that was central to the project. Since special demonstration funding was not available, the bulk of program operating support came from the nation's employment and training system authorized by the Job Training Partnership Act (JTPA). Thus, the JOBSTART model received a "real-world" test by operating within the existing mainstream delivery system. MDRC – a private, nonprofit corporation experienced in designing, overseeing, and evaluating innovative programs – conceived the demonstration and had overall responsibility for managing it and for conducting the evaluation.

This report, the third of four on the JOBSTART Demonstration, summarizes the key implementation lessons, including recruitment and participation patterns, and features the impact findings from a two-year follow-up survey. In this survey, the educational and labor market experiences of youths who were referred to JOBSTART were compared to those of a randomly selected control group. Overall, the results were mixed. The sites succeeded in implementing JOBSTART, and JOBSTART substantially increased educational attainment levels, as measured by receipt of a GED (high school equivalency certificate). However, the educational investment has not yet translated into employment and earnings gains, although the results are somewhat more encouraging for young women than for young men.

The report also provides insight into different ways of structuring education and training services – either sequentially or concurrently – but there were no clear patterns of impact differences based on how sites organized these services.

A final report, scheduled for 1993, will include the results of a four-year follow-up survey and thus clarify whether, over this longer period, the educational investment led to greater labor market success.

MDRC and the policy community at large owe a special debt of gratitude to the consortium of 12 demonstration funders, the program staff at the 13 participating sites, and the sites' state and local funding agents. They shared a commitment both to enhance the employment prospects of disadvantaged young people and to build a knowledge base so that future policies can be guided by facts and evidence rather than speculation. Learning what works in the social policy arena hinges on the formation of collaboratives such as this, and it is our hope that the structure of the JOBSTART Demonstration becomes a model for future endeavors aimed at alleviating poverty and increasing the self-sufficiency of disadvantaged populations.

Robert J. Ivry
Senior Vice President

EXECUTIVE SUMMARY

Although most young people move fairly smoothly from adolescence to employment and self-sufficiency, high school dropouts with poor skills are increasingly unable to make this transition. The statistics are stark: In 1990, only about one-half of all 16- to 24-year-olds who had not completed high school and were not enrolled in some type of education program were working. The figures for blacks are even more discouraging, showing less than one-third working.

The consequences of these employment problems reach well beyond the lives of the young people themselves. Employers and the business community as a whole suffer because job applicants lack the basic skills needed to perform productively. Furthermore, demographic trends suggest a possible worsening of this problem: Over the next decade, the number of young entry-level workers will remain basically stable while the economy continues to grow, and an increasing proportion of these young people will come from groups with historically higher-than-average school dropout rates and basic skills weaknesses. From the perspective of government budgets, these nonworking young people are using services but not paying taxes. Government and nonprofit agencies must contend with long-term welfare dependence, crime, and drug abuse, which are tied to lack of employment success in many direct and indirect ways.

The JOBSTART Demonstration addressed these issues by testing a program of basic education, occupational skills training, support services, and job placement assistance for young school dropouts who read below the eighth-grade level. The demonstration, which was developed and is being evaluated by the Manpower Demonstration Research Corporation (MDRC), was implemented between 1985 and 1988 in 13 diverse sites. Operating funds were provided primarily under the Job Training Partnership Act (JTPA), the nation's principal employment and training program for economically disadvantaged people.

This report, the third of four on the JOBSTART Demonstration, summarizes findings on the program's implementation (covered in detail in past reports) and presents early findings, based on two years of follow-up, on the difference that the program made in young people's educational attainment, employment, welfare receipt, and other important outcomes. A final report, based on approximately four years of follow-up, will present a more complete picture of program impacts and summarize findings on JOBSTART's cost-effectiveness.

The Goals of the JOBSTART Demonstration and a Summary of Interim Findings

Few programs have been shown to be effective in increasing the educational attainment, employment, and earnings of young people with poor skills. At the time JOBSTART was conceived, the evidence pointed toward more comprehensive programs, which attempt to remedy basic and occupational skills weaknesses, provide support services such as transportation and child care assistance, and substantially help participants find a job. JOBSTART was modeled after one of the few programs recognized to be a success story: the residential Job Corps. That program provides basic education, occupational training, job placement assistance, and an extensive array of support services to the participating youths, who live at Job Corps Centers.

However, the residential Job Corps cannot be offered to most young dropouts. It is relatively expensive, operates in specialized centers, requires the development of work experience positions with employers, and attracts only young people willing and able to live away from home. JOBSTART drew on the Job Corps' experience by offering most of the same basic components in a nonresidential program, although clearly the support services available in most demonstration sites were less extensive than those provided by the Job Corps. Also, there was no work experience component in JOBSTART.

The JOBSTART Demonstration sought to answer four key policy questions relating to this general programmatic approach.

- ***Recruitment*. Could local agencies recruit young, economically disadvantaged, poorly skilled school dropouts?**

Many program operators have discovered how difficult it is to reach alienated young people and provide them with the hope and support they need to participate in an intensive program such as JOBSTART. Young school dropouts are often reluctant to return to a school setting, require extensive support services to participate, or seek immediate employment to meet pressing financial needs. In addition, the lengthy eligibility determination process characteristic of many programs (including JTPA's) may discourage some of those who are initially interested.

- *Implementation.* **Could sites, working within the funding and administrative constraints of JTPA, put in place a package of services that would address the needs of these youths?**

Sites participating in JOBSTART had to raise operating funds from existing programs, and most relied on Title IIA of JTPA. When the demonstration began (and to a considerable extent at the present time), federal and state regulations and prevailing administrative practices encouraged local JTPA administrative agencies (called service delivery areas) to emphasize shorter-term, lower-cost programs and to enroll participants who were more employable than the JOBSTART target group. More specifically, JTPA's performance standards (which emphasized the proportion of participants placed in a job, their wages, and the cost per "success story") created incentives to choose people who were more likely than the JOBSTART target group to achieve these successes at a relatively modest cost. All these factors led to a clear result: At the time JOBSTART began, only about one in four young people served under Title IIA of JTPA were school dropouts, and only about one in four of these school dropouts received basic education. Thus, the successful implementation of JOBSTART (with its relatively lengthy and intensive program components and its disadvantaged target group) could not be taken for granted within JTPA.

- *Participation.* **Would the young people respond favorably to this opportunity and make an investment of their time and effort by participating in the services?**

Many youths need a substantial amount of education to improve their basic skills, and occupational training to instill job-related competencies, before they can be competitive in the job market. Yet the conditions that make it difficult to recruit them into education and training programs often preclude their completing the coursework. Financial pressures are severe, including the need for immediate cash to pay the rent and buy food. Since many disadvantaged young people work already – albeit sporadically and for very low wages – it is difficult to attract them into programs that cannot pay them stipends, as is the case under JTPA rules. Other barriers to participation – even in programs offering "free" training – include child care responsibilities, unstable housing arrangements, and peer pressure against participation. Programs must find ways to help "stabilize" the young people's lives so they can move beyond their immediate problems and commit themselves to invest in learning new skills with a longer-term payoff.

- *Impacts.* **Would the program lead to an increase in educational attainment, and would this in turn have an impact on subsequent employment, earnings, and other outcomes?**

The JOBSTART Demonstration provided a rigorous test of the difference that JOBSTART's combination of nonresidential services made in the lives of young people. Youths who applied for the program were randomly assigned to a group given access to JOBSTART (the experimental group) or to a group not given that access but free to seek other services in the community (the control group). Since the two groups were created by chance, using a lottery-like process, there was only one systematic difference between them: Only those in the experimental group could receive JOBSTART services. Thus, the control group provides information on what those in the experimental group would have done if there had been no JOBSTART program: Some would have found alternative services, some would have worked, and so forth. Therefore, a comparison of the two groups' behavior over time provides an estimate of the difference that the added services the experimental group received (that is, services above the level the control group received on its own) had on their subsequent employment, earnings, welfare receipt, and other outcomes.

Findings available at this half-way point in the follow-up period provide answers to the first three questions and a partial answer to the fourth:

- *Recruitment.* Through intensive outreach efforts, sites were able to recruit the target population of poorly skilled, economically disadvantaged young people, although several sites were unable to meet their recruitment goals.

- *Implementation.* With considerable special effort, sites were able to fund and operate the program components within the JTPA system, and the program model received a fair test in most sites.

- *Participation.* Participation by the experimental group in education and training was substantial and was much above that of the control group youths who sought similar types of services elsewhere. However, most youths fell short of the hours-of-attendance targets for the demonstration.

- *Impacts (on educational attainment).* During the 24 months of follow-up available for this report, JOBSTART led to a doubling of the rate of receipt of a high school diploma or GED (high school equivalency credential) relative to the control group. This was true for both the full sample and most subgroups.

Information on longer-run labor market outcomes is not yet available. The early findings may be summarized as follows:

- **Early impacts on employment.** As expected, more youths in the control group worked during the first year of follow-up than did those in the experimental group, for whom participation in JOBSTART was a major activity. In the second year of follow-up, approximately equal proportions of youths in the two groups worked.

- **Early impacts on earnings.** During the first year of follow-up, when a large portion of the experimentals were active in JOBSTART, controls' earnings, not surprisingly, exceeded experimentals' earnings by a significant amount. The average earnings of the experimental group remained below those of the control group during the second year. However, the gap in annual earnings showed an encouraging trend, narrowing from $585 in the first year to $205 in the second, and this second-year difference was not statistically significant.

- **Early impacts for key subgroups.** There were early differences among key subgroups, although smaller sample sizes make conclusions less certain. Among men, earnings of experimentals were significantly less than those of controls in both years of follow-up, although the difference narrowed. For women living with their own children and for other women, the trend was more favorable: After earning less than controls in the first year, experimentals in both categories of women earned slightly more than controls in the second year.

- **Impacts for individual sites.** Individual sites' impacts varied widely but, except for impacts on educational attainment, the differences across sites were not statistically significant. There was no clear relationship between individual sites' impacts and whether sites offered education before training or both services concurrently, or whether they provided all services themselves or arranged for some services to be provided by other agencies.

Later sections of this Executive Summary present these results in more detail, again grouping them under the topics of program recruitment, implementation, participation, and impacts.

The Structure of the JOBSTART Demonstration

The JOBSTART Demonstration guidelines specified the target group and the character of the core service components. The program was to target 17- to 21-year-old, economically disadvantaged school dropouts who read below the eighth-grade level and were eligible for JTPA Title IIA programs or the Job Corps (which is funded under Title IVB of JTPA). The four central program components were to be implemented as follows:

- **Instruction in basic academic skills** was to be based on individualized curricula chosen by the sites to allow youths to proceed at their own pace toward competency goals in reading, communication, and basic computational skills.

- **Occupational skills training** was to be given in a classroom setting combining theory and hands-on experience to prepare participants for jobs in high-demand occupations.

- **Training-related support services** were to include assistance with transportation and child care, counseling, and, where possible, additional support such as work-readiness and life skills (practical everyday knowledge) training and needs-based or incentive payments tied to program performance.

- **Job placement assistance** was to be provided to help JOBSTART youths find training-related jobs.

Sites were required to offer at least 200 hours of basic education and at least 500 hours of occupational training. These minimums were set after balancing two factors: (1) Youths in the JOBSTART target group were likely to need a substantial amount of education and training if they were to have real opportunities to become competitive in the job market, and (2) the administrative practices of JTPA made higher service targets unrealistic.

Within this general framework, the 13 local JOBSTART programs did vary, reflecting their diverse operating experiences, funding sources, clientele, and local service networks. Among the important types of local variation were: additional program entry requirements in some sites, the extent of integration of education and training instruction, whether the young people were taught in separate classes or by being "mainstreamed" in classes with adults, the number and duration of occupational training courses (small community-based organizations typically offered far fewer courses than did other sites), and the strength of the implementation of the core JOBSTART components, especially training and job placement assistance.

The sites are listed in Table 1. They included six community-based organizations, three adult vocational schools, a community college, and three Job Corps Centers that already operated nonresidential Job Corps programs.

Findings on Recruitment of Youths

Analysis of the characteristics of the 1,839 youths who made up the research sample for this report focused on three key questions: Did sites succeed in recruiting the demonstration's intended target group? Were these JOBSTART youths more or less disadvantaged than those served in other programs? Were there observed educational, employment, or other differences in the backgrounds of subgroups of youths (such as men, women living with their children, and other women) that might help explain differences in their participation and impacts?

- **With considerable effort, the sites recruited the poorly skilled, economically disadvantaged young people making up the intended target group for the demonstration.**

The youths in the demonstration were all eligible for JTPA Title IIA programs or the Job Corps; they included slightly more women than men. Most of the sample were members of minority groups and unmarried; nearly three-fourths were under 20 years of age; almost one-half did not work during the year prior to random assignment; and about three-fifths left school before the eleventh grade.

JOBSTART youths appear to have been more disadvantaged than the majority of young people – or even young dropouts – served nationwide by JTPA Title IIA programs during the period JOBSTART was in operation. However, those in JOBSTART, especially the men, were not among the most disadvantaged youths (young people who rarely participate in any program) and were probably slightly more skilled than the typical residential Job Corpsmember during the same period.

- **Among JOBSTART youths, women living with their own children had noticeably weaker ties to employment than did other women or men.**

Prior work experience could affect both the likely behavior of the youths in the control group and the service needs of participants in the program. Women living with their own

TABLE 1

THE JOBSTART SITES

Agency Name and Location	Type of Organization	Prior Service Emphasis[a]	JOBSTART Program Structure[b]
Allentown Youth Services Consortium, Buffalo, NY[c]	Community-based	Education	Sequential/brokered
Atlanta Job Corps, Atlanta, GA	Job Corps Center	Education and training	Concurrent
Basic Skills Academy (BSA), New York, NY	Community-based	Education	Sequential/brokered
Capitol Region Education Council (CREC), Hartford, CT	Community-based	Education	Sequential/brokered
Center for Employment Training (CET), San Jose, CA	Community-based	Training with some education	Concurrent
Chicago Commons Association's Industrial and Business Training Programs, Chicago, IL	Community-based	Training	Concurrent
Connelley Skill Learning Center, Pittsburgh, PA	Adult vocational school	Education and training	Concurrent
East Los Angeles Skills Center, Monterey Park, CA	Adult vocational school	Education and training	Concurrent
El Centro Community College Job Training Center, Dallas, TX[d]	Community college	Education and training	Sequential/in-house
Emily Griffith Opportunity School (EGOS), Denver, CO	Adult vocational school	Education and training	Concurrent
Los Angeles Job Corps, Los Angeles, CA	Job Corps Center	Education and training	Sequential/in-house
Phoenix Job Corps, Phoenix, AZ	Job Corps Center	Education and training	Concurrent
SER/Jobs for Progress, Corpus Christi, TX	Community-based	Training	Concurrent

NOTES: [a] Education refers to basic education, often as preparation for the GED examination. Training refers to instruction in occupational skills needed for specific jobs.

[b] *Concurrent* programs offer basic education and occupational training concurrently from the beginning of participation. *Sequential/in-house* programs offer basic education followed by occupational training, with both components provided in-house by the agency. *Sequential/brokered* programs provide basic education and then serve as a broker for occupational training, referring participants to other agencies.

[c] In October 1990 this site was renamed The Clarkson Center, Inc.

[d] In September 1988 this site was renamed the Edmund J. Kahn Job Training Center.

children were least likely to have worked during the year before random assignment and most likely to have received Aid to Families with Dependent Children (AFDC) and Food Stamps. Other women worked much more, and the men in the sample worked most of all. Because of clear differences in initial characteristics between mothers caring for their children and other women, the gender-based subgroup analyses that follow divide the female sample into these two groups. However, in most of the analysis, all men are grouped together because only a small number of men reported at the outset that they were parents.

Findings on Program Implementation

- **In general, the JOBSTART program model received a fair test in the demonstration. Most sites were able to put the program model in place, although the sites varied considerably in the intensity of their services and the way they were offered.**

Basic education, occupational training, support services, and job placement assistance were available to participants in each site, but the varying ways these services were linked reflected the sites' past operational experience and current capacity to provide services in-house. As shown in Table 1, eight of the 13 sites provided basic education and occupational skills training concurrently ("concurrent" sites); two provided a sequence of education followed by training ("sequential/in-house" sites); and three provided education and then referred participants to other agencies for training ("sequential/brokered" sites). In sites offering education and training concurrently, participants usually attended two hours of education classes and four hours of vocational training a day. In sites operating a sequential program, participants generally attended three hours a day of basic skills classes during the education phase, with the remaining three hours a day being devoted to life skills classes and employability workshops. As discussed later, participation rates and hours in the components of the model differed among these three types of sites.

Basic Education Activities

- **Sites successfully implemented the JOBSTART basic education component, although they varied in educational emphasis.**

The education component typically consisted of individualized instruction, which allowed students to move at their own pace learning reading, mathematics, and other subjects needed to pass the General Educational Development (GED) examination.

In sites where funding for education services was based on students passing the GED examination, staff made GED certification an important short-term goal and emphasized the skills tested on the GED examination in their education components. Two concurrent sites – CET/San Jose and Chicago Commons – provided some or all of their basic skills instruction as part of vocational training, with a focus on improving those skills needed to successfully complete the training. The remaining sites fell between these extremes.

Occupational Skills Training

- **Despite great variation in course offerings among the sites, JOBSTART youths generally studied occupations with skills requirements comparable to those for adults served within JTPA nationwide.**

Participants at large vocational schools could choose courses in more than 20 occupations, and the Job Corps Centers and large community-based organizations also offered a wide range of training. In contrast, small community-based organizations providing training in-house typically offered fewer than five courses. In theory, youths at sequential/brokered sites could choose courses from a variety of agencies, but in practice their choices were limited because they could not meet entrance requirements or encountered other administrative difficulties in gaining admission. Occupational choices for men and women followed traditional patterns, with about three-fourths of the women in clerical fields and slightly more than one-half of the men in machine trades, metal fabrication, or construction.

JOBSTART training was intended to prepare participants primarily for jobs requiring moderate skills (about one-half of participants) or higher skills (about one-fourth of participants). This distribution of skills ratings for training occupations was similar to what the U.S. General Accounting Office found for JTPA adult programs, an unexpected result since JOBSTART participants faced more barriers to employment than did the typical JTPA adult client.

Training-Related Support Services

- **All sites provided transportation and child care assistance, but the availability of other services varied greatly.**

Clearly, the support services and other activities available at the Job Corps Centers surpassed those at the other sites in both breadth and intensity. However, many sites were able to provide instruction in life skills and some type of needs-based payments.

Job Placement Assistance

- **The job placement component of the program was the least developed in many sites. In particular, participants leaving JOBSTART before completing the curriculum received relatively little aid in finding a job.**

In sites with strong job placement assistance, instruction in proper work behavior, employer expectations, and job search techniques began while students were still in training; placement specialists provided leads and assistance in finding jobs; and staff had especially strong ties to local employers. Job placement assistance was noticeably weaker in the three sequential/brokered sites, where it was the responsibility of the training agency. Unfortunately, most participants never reached the training components, so only informal assistance was available from the JOBSTART agency.

Findings on Participation in JOBSTART

- **Young people in the experimental group attended an average of more than 400 hours of group activities, which is impressive when compared to the experience of many other JTPA-funded programs. Despite this relative success, most young people did not acquire the skills needed to pass the GED examination and did not participate enough to complete an occupational training course.**

Experimentals (including the 11 percent who did not participate at all) averaged 415 hours of attendance in JOBSTART classes, as shown in Table 2. The young people spent most of this time in education and occupational training, as opposed to other activities such as life skills training. Slightly more than one-third of all experimentals participated for more than 500 hours, and another one-quarter were active for 201 to 500 hours. These findings show that JOBSTART succeeded in engaging more than one-half of the youths in the experimental group in the program and its activities, but for about two-fifths of those in the experimental group, participation was neither lengthy nor intensive.

The average length of stay in JOBSTART was 6.8 months, with 16 percent of experimentals still active in the program 12 months after random assignment and nearly 10 percent active 15 months after random assignment. This means that for most experimentals

TABLE 2

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS

Activity Measure	Experimentals
Percent participating in	
Any activity	88.7
Education	85.9
Training	66.6
Education and training	64.4
Other activities	40.0
Average hours in	
Education	128.1
Training	248.9
Education and training	377.0
Other activities	37.3
All activities	414.8
Percentage distribution of hours in education and training	
None	11.9
Up to 200	33.2
201 to 500	22.4
501 to 700	15.5
701 or more	17.0
Total	100.0
Percentage distribution of hours in all activities	
None	11.3
Up to 200	28.6
201 to 500	25.5
501 to 700	15.2
701 or more	19.5
Total	100.1
Length of stay (months)	
Average	6.8
Median	6.0
Percent still participating in month	
3	78.0
6	53.6
9	30.6
12	16.4
15	9.6
18	4.8
19 or later	3.7
Number of experimentals	949

NOTES: Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

the first year of follow-up was primarily a period of program participation and that for 16 percent the second year also included months with program activity. JOBSTART experimentals stayed in the program considerably longer than the typical young dropout served in JTPA Title IIA programs and nearly as long as the average Job Corpsmember.

- **Differences in participation were associated with choices made at the site level (1) to offer concurrent versus sequential education and training and (2) to house all services on-site rather than refer youths elsewhere for training.**

Youths in sequential/in-house sites averaged the highest total hours, followed by youths in concurrent programs. Those in sequential/brokered sites averaged noticeably fewer total hours, primarily because only about 25 percent of the young people successfully made the transition to the off-site occupational training. This occurred because of difficulties arranging linkages with another organization for training within JTPA and because youths at these education-oriented sites may have been more interested in studying for their GED credential than in getting occupational training. (Young people in sequential/brokered sites averaged the most education hours.) As would be expected, sequential sites showed a higher proportion of experimentals still active in the program a year after random assignment.

- **Participation was surprisingly similar among key demographic groups.**

Men, women living with their own children, and other women averaged 410, 408, and 429 total hours, respectively. The groups' average lengths of stay in the program were quite similar, although a somewhat higher proportion of women than men were still active in JOBSTART 12 months after random assignment. Youths who had not been arrested between their sixteenth birthday and the time of random assignment did have significantly higher average hours of participation. However, other subgroup analysis (such as by race, age, grade level at school dropout, marital status, and receipt of public assistance) showed no statistically significant differences in average total hours.

Findings on Program Impacts

Young people in the JOBSTART sample could have improved their skills by several means: through participating in JOBSTART (for those in the experimental group) or in other education and training programs, or by learning on the job. Program impacts are measured by comparing the experiences of the experimental and control groups; in essence, this compares the payoff of the investment made by experimentals with that made by controls.

A Framework for Analyzing JOBSTART's Labor Market Impacts

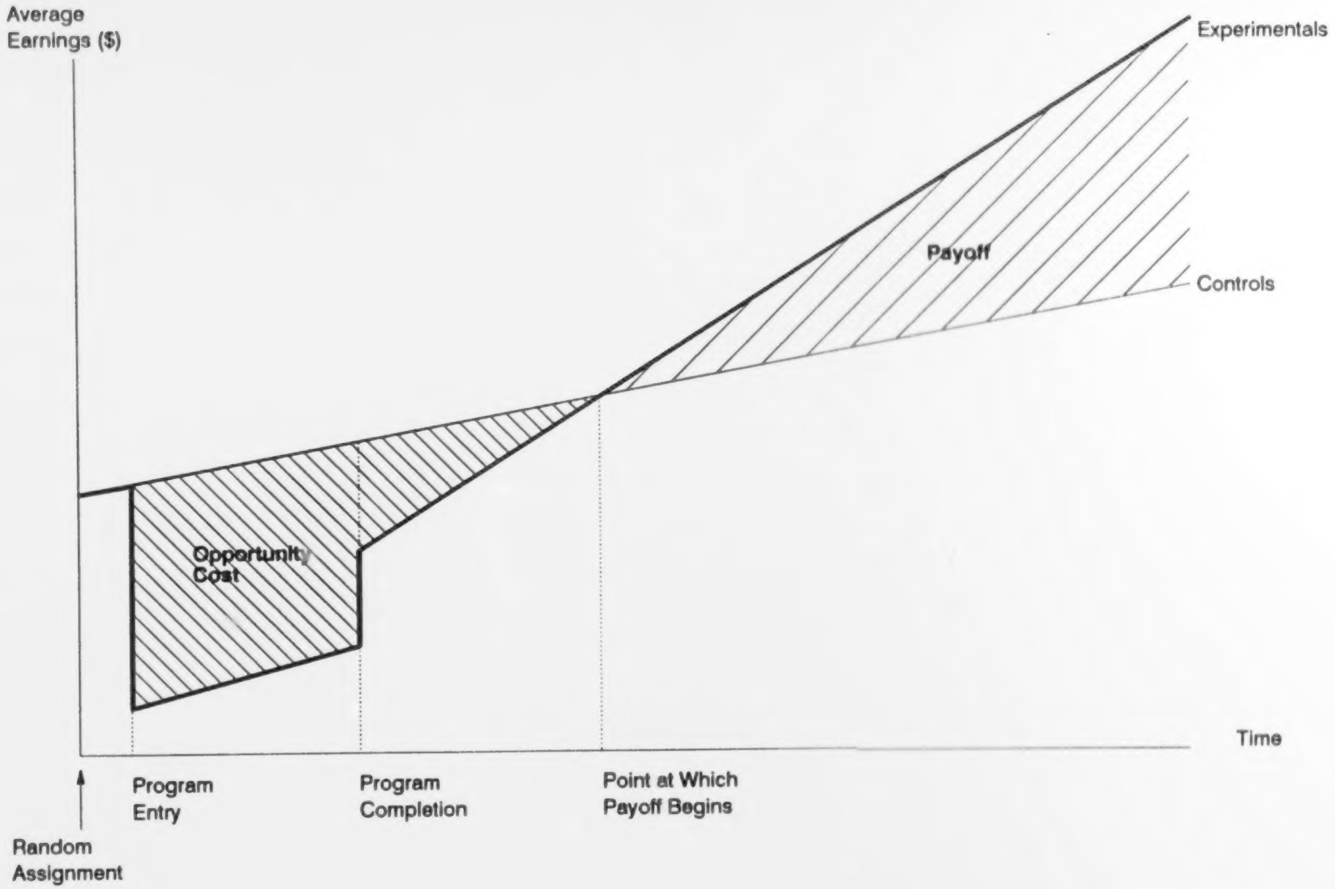
As is true of most investments, many of the costs of participating in a program such as JOBSTART are incurred in the short run. The benefits will accrue over the young people's lifetime if they learn new skills that pay off in the labor market. Figure 1 presents a theoretical framework for analyzing the labor market aspects of this investment and the alternative investments made by young people in the control group. Those in JOBSTART committed their time and effort to improving their skills in the expectation of a future payoff. While participating, they gave up the chance to work and earn, so any forgone income was an in-program opportunity cost of the program, represented in the figure by the shaded area between the start and end of JOBSTART participation.

The figure also shows the control group's earnings rising over time. This reflects their growing employment rate and, for some, the acquisition of new skills on the job. For young people with poor skills, work experience can be an important source of new skills, which can translate into increased productivity and earnings and more stable employment. Once their participation in JOBSTART ended, young people in the experimental group will have looked for jobs, but their employment rates and earnings may not immediately exceed (or even reach) those of controls who were already working rather than attending a program. This post-program opportunity cost is the area in the figure between the end of program participation and the hoped-for point at which the earnings of the experimental group exceed those of the control group.

For a program such as JOBSTART to pay off for young people, the long-term benefits of increased education and training (represented in the figure by the shaded area on the right) must exceed the forgone – more immediate – rewards of possible earnings and enhanced skills through work experience. Even in successful programs, it will take time for participants to overcome the head start of those who have been working throughout the program period.

FIGURE 1

A THEORETICAL VIEW OF THE PAYOFF OF A PERSONAL INVESTMENT IN EDUCATION AND TRAINING



The Two-Year Impact Sample

The 2,312 youths who applied for JOBSTART and were judged eligible were randomly assigned to either the experimental or control group. Follow-up surveys attempted to reach all members of both groups 12 and 24 months after they were randomly assigned. This analysis of program impacts uses a sample of 1,839 (80 percent of all those who were randomly assigned). These are the youths who provided information for this two-year period. Impacts reported in the following sections were statistically significant (that is, unlikely to have arisen by chance) unless otherwise noted.

Experimental-Control Differences in Participation in Education and Training

For JOBSTART to make a difference in the lives of the young people, those in the experimental group must have participated in substantially more education and training activities than did those in the control group, who had access to other services in the community.

- **During the follow-up period, experimentals were nearly twice as likely as controls to have participated in some type of education or training. However, nearly one-half of all controls also got some education or training.**

Table 3 shows experimental-control differences in the percentage of the youths who ever participated in education and training; the differences are shown for both the full sample and key subgroups. In the two years following random assignment, 93 percent of experimentals versus 44 percent of controls received some type of education or training. Experimentals averaged 619 hours in these activities, whereas controls averaged 250 hours. The differences were largest during the early months of the follow-up period, when most experimentals were active in JOBSTART, and gradually disappeared by the end of the two years. All the subgroups that were analyzed showed large differences between experimentals and controls in service receipt. There was a greater experimental-control difference in the percentage of youths participating in education than in the percentage participating in training, because many young people at sequential (and especially sequential/brokered) sites did not participate in training.

TABLE 3

IMPACTS OF JOBSTART ON SERVICE RECEIPT AND EDUCATIONAL ATTAINMENT DURING THE 24 MONTHS FOLLOWING RANDOM ASSIGNMENT

Outcome and Subgroups	Sample Size	Experimentals	Controls	Difference
Ever received any education or training				
Full sample	1,839	92.7%	44.2%	48.4***
Men	871	92.8	36.6	56.1***
Women living with own child(ren)	484	93.6	47.9	45.7***
Women not living with own child(ren)[a]	484	92.5	54.1	38.4***
Received GED or high school diploma				
Full sample	1,839	33.2	16.4	16.7***
Men	871	32.1	16.9	15.2***
Women living with own child(ren)	484	35.5	14.2	21.3***
Women not living with own child(ren)[a]	484	32.4	18.2	14.2***

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] Includes women who did not have children.

Impacts on Educational Attainment

- **JOBSTART led to a doubling of the rate of GED certification or receipt of a high school diploma, from 16.5 percent of the control group to 33.1 percent of the experimental group.**

Table 3 presents JOBSTART's impacts on educational attainment during the 24 months of follow-up and includes both the full sample and key subgroups. The full sample impact on attainment of a GED or high school diploma was 16.7 percentage points, similar to the results found in an evaluation of the residential Job Corps.

- **These large educational attainment impacts were present for many different subgroups in the overall sample.**

Among men, 32.1 percent of experimentals versus 16.9 percent of controls completed high school or passed the GED examination during the follow-up period, for an impact of 15.2 percentage points. For women living with their own children, the figures were 35.5 percent of experimentals and 14.2 percent of controls, for an impact of 21.3 percentage points; and for other women, the figures were 32.4 percent of experimentals and 18.2 percent of controls, for an impact of 14.2 percentage points. Numerous other subgroups based on work experience, welfare receipt, prior education, initial reading level, and age all showed similar large impacts.

Impacts on Employment and Earnings for the Full Sample

- **As expected, more youths in the control group than in the experimental group worked during the first year of follow-up. In the second year of follow-up, the proportions were not significantly different.**

As shown in Table 4, 57.6 percent of experimentals and 61.6 percent of controls worked at some time during the first year of follow-up, for a 4.0 percentage point decrease in employment among experimentals relative to controls. At some point during the second year, 69.5 percent of controls and 72.0 percent of experimentals worked. This 2.5 percentage point impact was not statistically significant. The control group's employment rate exceeded the experimental group's by the largest amount in month 5, after which the difference narrowed. In the second year of follow-up, the experimentals' employment rate exceeded the controls' rate in each of six months and was below it in the other six months; however, in no month was this difference statistically significant.

TABLE 4

IMPACTS OF JOBSTART ON EMPLOYMENT AND EARNINGS DURING THE 24 MONTHS FOLLOWING RANDOM ASSIGNMENT

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference
Full sample			
Ever employed (%)			
Months 1-24	79.2	77.9	1.3
Months 1-12	57.6	61.6	-4.0*
Months 13-24	72.0	69.5	2.5
Total earnings ($)			
Months 1-24	5,859.56	6,649.07	-789.52***
Months 1-12	1,965.47	2,550.26	-584.78***
Months 13-24	3,894.08	4,098.81	-204.73
Sample size	949	890	
Men			
Ever employed (%)			
Months 1-24	89.0	90.1	-1.1
Months 1-12	66.7	74.8	-8.1***
Months 13-24	84.3	84.3	0.0
Total earnings ($)			
Months 1-24	7,797.22	9,492.61	-1,695.39***
Months 1-12	2,648.50	3,676.88	-1,028.38***
Months 13-24	5,148.72	5,815.73	-667.01*
Sample size	438	433	
Women living with own child(ren)			
Ever employed (%)			
Months 1-24	62.1	57.2	4.9
Months 1-12	41.0	38.3	2.7
Months 13-24	53.3	46.1	7.2
Total earnings ($)			
Months 1-24	3,035.55	2,952.31	83.24
Months 1-12	1,011.83	1,100.96	-89.13
Months 13-24	2,023.73	1,851.35	172.38
Sample size	250	234	
Women not living with own child(ren)[a]			
Ever employed (%)			
Months 1-24	79.3	75.8	3.4
Months 1-12	57.0	61.5	-4.5
Months 13-24	69.5	65.3	4.2
Total earnings ($)			
Months 1-24	5,070.23	5,290.80	-220.58
Months 1-12	1,607.50	2,030.02	-422.53*
Months 13-24	3,462.73	3,260.78	201.95
Sample size	261	223	

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] Includes women who did not have children.

- **Experimentals earned significantly less than controls in the first year of follow-up. In the second year, the gap between experimentals and controls narrowed, and the difference was no longer statistically significant.**

As expected, experimentals earned less than controls during the first year of follow-up (see Table 4); this $585 difference was a clear opportunity cost of participating in the program. In the second year, although the proportion of experimentals working drew even with the proportion of controls, experimentals continued to lag slightly (but not significantly in the statistical sense) behind controls in hours worked per week and weeks worked per month. As a result, the earnings of experimentals remained below those of controls during the second year; however, the difference ($205) was no longer statistically significant. The cumulative opportunity cost in the form of forgone earnings was, therefore, $790.

Impacts on Employment and Earnings for Key Subgroups

Many past studies of nonresidential education and training programs have found starkly different results for men, women living with their children, and other women. Thus, it is important to move behind the findings for the full sample and examine subgroups that are of special policy interest. In this analysis, sample sizes are smaller and the conclusions are therefore less certain.

- **There were early differences among key subgroups. For men, earnings impacts were negative throughout the two-year follow-up period, while for women there were signs of a favorable trend.**

Table 4 shows employment rate and earnings impacts for these subgroups. Among men, a significantly lower percentage of experimentals than controls worked at some point in the first year, but in the second year employment rates were equal. Earnings for experimentals were significantly below those of controls in both years, although the gap did narrow. For the two groups of women, the pattern over time was more positive, but sample sizes were smaller, so almost all of the impacts were not statistically significant. Among women living with their own children at the time of random assignment, a higher percentage of experimentals than controls worked in each of the two years, with the second year showing a somewhat larger employment rate impact. For this subgroup, controls earned slightly more than experimentals during the first year, while experimentals earned more during the second, resulting in a small positive earnings impact for the two-year period. For other women, a higher percentage of controls than experimentals worked at some point in the first year, while in the second year, a higher percentage of experimentals worked. Reflecting this, the earnings of controls exceeded those of experimentals (by a statistically significant amount) in the first year; in the second year, experimentals' earnings were higher than controls' earnings, but the two-year total for earnings was slightly negative.

- **The employment experience of the controls in the three groups provides much of the explanation for the pattern of impacts among the groups.**

One likely explanation for better employment results for women is that it is easier to *improve* the employment and earnings of those who do not spend much time in the world of work (for example, young mothers) than of those who are already in the labor force but fail to find and keep steady, well-paying jobs (for example, poorly skilled young men). Thus, from this perspective, women have greater potential to improve their labor market outcomes than do men, and less to lose (in terms of forgone employment and earnings) by investing in education and training.

The JOBSTART sample followed this pattern, as shown in Table 4. During the first year after random assignment, 74.8 percent of control men worked at some point, compared to 38.3 percent of women living with their own children and 61.5 percent of other women. During this period, which, for many experimentals, consisted mostly of program participation, the impact of JOBSTART on employment rates was 2.7 percentage points for the young mothers, -4.5 percentage points for other women, and -8.1 percentage points for men. Men, and women who were not caring for children, thus paid an opportunity cost for JOBSTART in terms of forgone employment, while young mothers did not.

- **The occupational distribution of training for men and women in JOBSTART may have also contributed to these differences in impacts.**

A second possible explanation for the poorer impacts observed for men in JOBSTART is the greater difficulty of placing men in jobs that reward a GED. Many women in JOBSTART, for example, were trained in clerical occupations and sought that type of work, a domain in which educational credentials are important. However, young men were more likely to find work in occupations that did not reward the GED, at least not in the early stages of employment. Many types of blue-collar work, especially physically demanding work, may well fall into this category. The problem for men could be further exacerbated by the greater difficulty of finding training-related employment for men who do study occupations in which having a GED matters.

Impacts on Other Outcomes for the Full Sample

- **During the first 24 months of follow-up, JOBSTART had no statistically significant impacts on a variety of other outcomes.**

This general conclusion was true for the full sample and both women and men, and applies to a range of outcomes including receipt of most public benefits, childbearing, fathering of children, provision of child support (by noncustodial parents), and criminal arrests. The lack of impacts on criminal arrests differs from the finding in a study of the residential Job Corps program, but there is an obvious reason to expect much less of an early impact on criminal behavior for JOBSTART. In the residential Job Corps program, young people moved from their own communities to special centers that provided the education and training services. Many of these centers are in isolated areas or in communities without large gang populations or heavy involvement of youths in the drug trade. Much of the Job Corps' impact on criminal behavior came during the in-program period because of this "isolation" effect. In JOBSTART, no such change took place in the young people's lives; they continued to live in their own neighborhoods and to spend time outside the program with their existing circle of friends.

Site Differences in Impacts

There is strong policy interest in the influence of program characteristics on impacts. However, it is extremely difficult to draw clear lessons from differences in the impacts observed for different types of JOBSTART sites. This study was not designed to analyze how variations in the way the basic JOBSTART model was implemented may have affected impacts. Youths were randomly assigned to the experimental or control group in each site, rather than to different types of programs, which would be necessary for a rigorous experimental test of the effectiveness of alternative approaches.

Attempts to use differences in impacts among the sites to understand the influence of program characteristics face serious problems. The number of sites was limited. Sample sizes in the individual sites were small, so most experimental-control differences within a site were not statistically significant. Furthermore, when the impacts in each site are compared, the differences among the site-specific impacts are not significant on most outcomes. Finally, the sites differed in many characteristics including the background of the youths in the sample, labor market conditions, the level of alternative services received by controls, and many dimensions of their JOBSTART programs.

Despite these difficulties, if a clear pattern of impacts across the sites emerged, it might still be possible to draw some tentative conclusions. For example, if most sites operating sequential programs of education followed by training had impacts that differed markedly from those of sites offering education and training concurrently from the start of program participation, one might conclude that this difference in program structure was an important factor in understanding the differences in impacts.

- **There was no clear pattern of impacts among the 13 sites that supports conclusions about the effect of program characteristics on program impacts.**

This general point can be illustrated using site-specific impacts on second-year earnings. One aspect of program design that is of special interest is the choice of sequential programs of education followed by training versus concurrent programs. When the JOBSTART sites are divided into two groups based on this program feature, within each group there are sites with both positive and negative earnings impacts. When the sequential programs are further subdivided into those offering all services themselves and those linking up with other agencies for some program components, within each of these groups there are both positive and negative impacts. To sum up the site story on this outcome: Very different types of programs had positive impacts. One program with positive earnings impacts was oriented toward training and offered education primarily in the context of occupational training. This program also had a strong job placement effort, little initial screening of applicants, and a relatively short length of participation. A second, in contrast, placed a heavy emphasis on basic education, and only one-third of its participants were active in any occupational training. It had a relatively weak job placement effort and lengthy participation. A third program, between these two extremes, also had positive impacts on second-year earnings.

Next Steps in the Research

The JOBSTART Demonstration tackled a difficult issue, the employment problems of poorly skilled, economically disadvantaged young people. This report provides early follow-up on how a program of education and training, support services, and job placement assistance will affect their employment and earnings. When these young people entered the demonstration, most were teenagers. The four-year follow-up survey, now in progress, will carry the JOBSTART story to the youths' early twenties, the time when most young people begin to apply their skills in a more serious and committed way to employment.

The program impacts reported here compared two different kinds of skills-enhancing investments. The experimental group, most of whom invested in JOBSTART, paid a clear initial cost in forgone employment in the hope of future payoffs in employment and earnings. Although some members of the control group were in education and training programs, controls were more likely than experimentals to be working; many were, in the process, learning new skills on the job. For these controls, the initial cost in forgone income was small or nonexistent. A long follow-up period is needed for a valid comparison of these two very different types of investments; if the follow-up is too short, the large initial costs of JOBSTART will be included, but not the chance to see if the payoff appears over time. Of course, as the long search for effective programs for disadvantaged young people illustrates, there is no guarantee that the payoff will occur.

Longer follow-up could be especially important in addressing two issues. Sites differed in the extent to which they emphasized education versus occupational training within JOBSTART. Training is more likely than education to have an immediate payoff if placement in a training-related job can be arranged. But education might have longer-run payoffs if it produces the basic skills needed to learn new skills in the future. The longer follow-up will provide an opportunity to understand the terms of this likely trade-off in program emphasis.

Longer follow-up may also prove useful in helping to explain the clear difference in short-term impacts for men and women. Young men, especially minority men, appear to have a more difficult time than women in moving into jobs that reward better basic skills and that relate to their occupational training. Longer-term follow-up will provide a chance to see whether, over time, they can make this transition.

CONTENTS

PAGE

ACKNOWLEDGMENTS iii
PREFACE v
EXECUTIVE SUMMARY vii
TABLES AND FIGURES xxxiii
ABBREVIATIONS xxxviii

CHAPTER

1 AN OVERVIEW OF THE JOBSTART DEMONSTRATION 1

- I. The Youth Employment Problem and the Challenge of Serving Young School Dropouts 5
- II. The Changing Programmatic Context of the JOBSTART Demonstration 12
- III. The JOBSTART Demonstration 16
- IV. The JOBSTART Evaluation and the Organization of This Report 27

2 STUDY DESIGN AND SAMPLE CHARACTERISTICS 30

- I. An Overview of the Study Design 30
- II. The Characteristics of the JOBSTART Youths 38

3 AN OVERVIEW OF JOBSTART SERVICES AND PARTICIPATION . . . 57

- I. The Nature of JOBSTART Services 58
- II. The Intensity of JOBSTART Participation 75
- III. Moving Behind the Aggregate Participation Measures 81
- IV. Summary of the Features of the Sites 93

4 RECEIPT OF EDUCATION AND SKILLS TRAINING AND JOBSTART'S IMPACTS ON EDUCATIONAL ATTAINMENT 96

- I. Receipt of Education and Training by Experimentals and Controls 97
- II. Impacts on Educational Attainment 114

PAGE

5 IMPACTS ON EMPLOYMENT, EARNINGS, AND OTHER OUTCOMES 125

I. JOBSTART as an Investment 126
II. JOBSTART's Impacts on Employment Rates 128
III. JOBSTART's Impacts on Earnings 141
IV. Moving Behind These Employment-Related Impacts 144
V. JOBSTART's Impacts on Positive Activity 154
VI. JOBSTART's Impacts on Other Outcomes 155

6 SITE VARIATION IN IMPACTS 161

I. A Framework for Analyzing Site Variation in Impacts 164
II. Full Sample Impacts, by Site 174
III. Subgroup Impacts, by Site 179
IV. Summary and Future Impacts To Watch 181

APPENDICES

A. Data Sources for the Evaluation 185
B. Impacts of JOBSTART: Methodological Issues 195
C. Supplemental Tables to Chapter 2 215
D. Supplemental Table to Chapter 3 227
E. Cost of the JOBSTART Program 237
F. Supplemental Tables to Chapters 4 and 5 251
G. Supplemental Tables to Chapter 6 261

REFERENCES 271

SELECTED MDRC PUBLICATIONS ON YOUTH PROJECTS 275

TABLES AND FIGURES

TABLE PAGE

1 THE JOBSTART SITES xiv

2 PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS xviii

3 IMPACTS OF JOBSTART ON SERVICE RECEIPT AND EDUCATIONAL ATTAINMENT DURING THE 24 MONTHS FOLLOWING RANDOM ASSIGNMENT xxiii

4 IMPACTS OF JOBSTART ON EMPLOYMENT AND EARNINGS DURING THE 24 MONTHS FOLLOWING RANDOM ASSIGNMENT xxv

1.1 LABOR FORCE STATUS AND EMPLOYMENT RATES FOR 16- TO 24-YEAR-OLDS NOT ENROLLED IN SCHOOL IN 1990 6

1.2 THE JOBSTART SITES 18

1.3 THE JOBSTART PROGRAM GUIDELINES 21

2.1 CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY SITE 40

2.2 CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY PROGRAM STRUCTURE 49

2.3 CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY GENDER AND PARENTAL STATUS 53

3.1 CHARACTERISTICS OF JOBSTART ACTIVITIES, BY SITE 59

3.2 PERCENTAGE DISTRIBUTION OF OCCUPATIONS FOR PARTICIPANTS IN TRAINING, BY GENDER 65

3.3 BASIC SUPPORT SERVICES AVAILABLE IN JOBSTART, BY SITE 68

3.4 RATINGS OF THE IMPLEMENTATION OF JOBSTART COMPONENTS, BY SITE 74

3.5 PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS 76

TABLE **PAGE**

3.6 PARTICIPATION AND LENGTH OF STAY FOR YOUNG DROPOUTS IN JTPA TITLE IIA, BY ACTIVITY 79

3.7 PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY, BY GENDER AND PARENTAL STATUS . . . 82

3.8 AVERAGE TOTAL PARTICIPATION HOURS, BY SELECTED CHARACTERISTICS OF EXPERIMENTALS AT THE TIME OF RANDOM ASSIGNMENT 85

3.9 PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS, BY SITE 87

3.10 PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY, BY PROGRAM STRUCTURE 90

3.11 SUMMARY OF JOBSTART IMPLEMENTATION, BY SITE 94

4.1 IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 24 99

4.2 IMPACTS ON TOTAL HOURS OF EDUCATION OR TRAINING RECEIVED THROUGH MONTH 24 102

4.3 IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS 105

4.4 IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 24, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT 108

4.5 IMPACTS ON EDUCATIONAL ATTAINMENT THROUGH MONTH 24 116

4.6 IMPACTS ON EDUCATIONAL ATTAINMENT THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS 117

4.7 IMPACTS ON GED ATTAINMENT THROUGH MONTH 24, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT 120

5.1 IMPACTS ON EMPLOYMENT RATES THROUGH MONTH 24 129

5.2 IMPACTS ON EMPLOYMENT RATES THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS 132

TABLE **PAGE**

5.3 IMPACTS ON YEAR-TWO EMPLOYMENT RATES, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT 135

5.4 IMPACTS ON EARNINGS THROUGH MONTH 24 142

5.5 IMPACTS ON EARNINGS THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS 145

5.6 IMPACTS ON YEAR-TWO EARNINGS, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT 147

5.7 IMPACTS ON WEEKS EMPLOYED PER MONTH AND HOURS WORKED PER WEEK THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS 152

5.8 IMPACTS ON CRIMINAL ARRESTS THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS 159

5.9 IMPACTS ON PREGNANCY AND CHILDBIRTH THROUGH MONTH 24, BY PARENTAL STATUS AT THE TIME OF RANDOM ASSIGNMENT 160

6.1 SUMMARY OF JOBSTART IMPLEMENTATION, BY SITE 165

6.2 EXPERIMENTAL-CONTROL DIFFERENCE IN SERVICE RECEIPT, BY SITE 171

6.3 SELECTED IMPACTS OF JOBSTART, BY SITE (NOT ADJUSTED FOR SITE DIFFERENCES IN SAMPLE CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT) 175

6.4 SELECTED IMPACTS OF JOBSTART, BY SITE (ADJUSTED FOR SITE DIFFERENCES IN SAMPLE CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT) 176

A.1 ACTIVITIES INCLUDED IN PARTICIPATION HOURS, BY SITE AND COMPONENT 188

A.2 RESPONSE RATES FOR 24-MONTH SURVEY, BY SITE 191

B.1 CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY RESEARCH STATUS 196

B.2 ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF ASSIGNMENT TO THE EXPERIMENTAL GROUP 199

TABLE PAGE

B.3 ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF UNIT SURVEY RESPONSE 203

B.4 ESTIMATED REGRESSION COEFFICIENTS FOR SELECTED OUTCOMES 208

C.1 CHARACTERISTICS IN THE YEAR BEFORE RANDOM ASSIGNMENT, BY EMPLOYMENT STATUS 217

C.2 CHARACTERISTICS IN THE YEAR BEFORE RANDOM ASSIGNMENT, BY GENDER AND EMPLOYMENT STATUS 220

C.3 CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY AGE 223

D.1 RATINGS AND SPECIAL FEATURES OF THE IMPLEMENTATION OF JOBSTART COMPONENTS, BY SITE 229

E.1 AVERAGE MONTHLY AND TOTAL COSTS PER EXPERIMENTAL AND PER PARTICIPANT, BY SITE 242

E.2 AVERAGE JOBSTART OPERATING COSTS PER EXPERIMENTAL, BY SITE 247

E.3 AVERAGE JOBSTART SUPPORT SERVICES AND PARTICIPANT PAYMENT COSTS PER EXPERIMENTAL, BY SITE 249

F.1 IMPACTS ON MONTHLY RECEIPT OF EDUCATION OR TRAINING 253

F.2 IMPACTS ON MONTHLY AVERAGE HOURS OF EDUCATION OR TRAINING RECEIVED 254

F.3 IMPACTS ON MONTHLY RECEIPT OF BASIC EDUCATION 255

F.4 IMPACTS ON MONTHLY RECEIPT OF TRAINING 256

F.5 IMPACTS ON MONTHLY EMPLOYMENT RATES, BY GENDER AND PARENTAL STATUS 257

F.6 IMPACTS ON MONTHLY RATES OF POSITIVE ACTIVITY, BY GENDER AND PARENTAL STATUS 259

G.1 EXPERIMENTAL-CONTROL DIFFERENCE IN SERVICE RECEIPT, BY SITE 263

G.2 IMPACTS ON YEAR-TWO EMPLOYMENT RATES AND EARNINGS FOR MEN, BY SITE 264

TABLE PAGE

G.3 IMPACTS ON YEAR-TWO EMPLOYMENT RATES AND EARNINGS FOR WOMEN LIVING WITH THEIR OWN CHILD(REN) AT THE TIME OF RANDOM ASSIGNMENT, BY SITE 266

G.4 IMPACTS ON YEAR-TWO EMPLOYMENT RATES AND EARNINGS FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN) AT THE TIME OF RANDOM ASSIGNMENT, BY SITE 268

FIGURE

1 A THEORETICAL VIEW OF THE PAYOFF OF A PERSONAL INVESTMENT IN EDUCATION AND TRAINING xxi

2.1 THE JOBSTART EVALUATION DESIGN AND SAMPLE FLOW 32

4.1 MONTHLY RECEIPT OF EDUCATION OR TRAINING, BY RESEARCH STATUS 99

4.2 MONTHLY AVERAGE HOURS OF EDUCATION OR TRAINING RECEIVED, BY RESEARCH STATUS 102

4.3 MONTHLY RECEIPT OF BASIC EDUCATION, BY RESEARCH STATUS 113

4.4 MONTHLY RECEIPT OF TRAINING, BY RESEARCH STATUS 115

5.1 MONTHLY EMPLOYMENT RATES, BY GENDER AND PARENTAL STATUS 133

5.2 MONTHLY RATES OF POSITIVE ACTIVITY FOR MEN, BY RESEARCH STATUS 156

5.3 MONTHLY RATES OF POSITIVE ACTIVITY FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS 156

5.4 MONTHLY RATES OF POSITIVE ACTIVITY FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS 157

ABBREVIATIONS

AFDC	Aid to Families with Dependent Children
BSA	Basic Skills Academy (New York, New York)
CBO	Community-Based Organization
CCP	Comprehensive Competencies Program
CET	Center for Employment Training (San Jose, California)
CETA	Comprehensive Employment and Training Act (1973)
CREC	Capitol Region Education Council (Hartford, Connecticut)
EGOS	Emily Griffith Opportunity School (Denver, Colorado)
FSA	Family Support Act (1988)
GAO	U.S. General Accounting Office
GED	General Educational Development Certificate
JOBS	Job Opportunities and Basic Skills Training Program
JTPA	Job Training Partnership Act (1982)
MDRC	Manpower Demonstration Research Corporation
MFSP	Minority Female Single Parent Demonstration
MIS	Management Information System
PIC	Private Industry Council
SDA	Service Delivery Area
SSI	Supplemental Security Income
TABE	Test of Adult Basic Education
VICI	Ventures in Community Improvement
WIN	Work Incentive Program
YIEPP	Youth Incentive Entitlement Pilot Projects
YOU	Youth Opportunities Unlimited Demonstration

CHAPTER 1

AN OVERVIEW OF THE JOBSTART DEMONSTRATION

Most young people make the transition from adolescence to employment and self-sufficiency between the ages of 16 and 24. However, many others – especially high school dropouts with poor skills – fail to do so, even in a period of strong economic growth such as the mid to late 1980s. The negative consequences extend well beyond the lives of the young people, affecting both the general public and the business community. There is strong evidence that the incidence of poverty, welfare receipt, criminal activity, and unwed parenthood is significantly higher for those with poor basic skills than for the population as a whole.[1] Society bears the cost of this in the form of social disruption and increased public services.

Employers and the business community also suffer from young people's lack of job-related skills, and they are likely to find this an even greater problem in the future. The U.S. Department of Labor projects that in the year 2000 the number of young people in the labor force – a major source of entry-level workers – will be basically unchanged, even though the economy as a whole will have expanded considerably.[2] Further, a growing proportion of these young people will come from groups with traditionally higher-than-average school dropout rates and basic skills deficiencies – minorities, recent immigrants, youths from single-parent families, and the poor. Basic skills shortages could translate into jobs going unfilled, employers having to pay higher wages to attract skilled workers, and some jobs needing restructuring to accommodate the available work force.

What will help disadvantaged young school dropouts lead more productive lives? A number of experts have called for programs of education and training.[3] The JOBSTART Demonstration tested such a program by combining basic education, training in occupational skills, limited support services (primarily assistance with child care and transportation), and job placement assistance. Developed and overseen by the Manpower Demonstration Research Corporation (MDRC), JOBSTART was implemented in 13 sites: four adult schools (three adult vocational schools and one community college), six community-based organizations (CBOs), and three nonresidential Job Corps programs. The demonstration ran from 1985 to 1989, with operating support consisting primarily of funds provided under the Job Training Partnership Act (JTPA), the nation's largest funder of employment and training programs for economically disadvantaged persons.[4]

---

[1] Berlin and Sum, 1988.
[2] Fullerton, 1989.
[3] William T. Grant Commission on Work, Family and Citizenship, 1988; Public/Private Ventures, 1990; Job Training Partnership Act Advisory Panel, 1989.

For an employment program like JOBSTART to improve the lives of young people, certain things must happen:

- The sites must recruit young, economically disadvantaged school dropouts with poor skills (a group that is typically not served even in intensive programs).
- The sites must put in place a package of services that addresses the needs of the youths.
- The young people must respond favorably to this opportunity and make an investment of their time and effort by participating in education and training activities.
- Their efforts must yield them new skills, as measured, for example, by their completing high school or passing the General Educational Development (GED) Test for high school equivalency certification.[5]
- Over time, these new skills must translate into greater employment and earnings than these youths otherwise would have had, and less need to rely on public assistance.[6]

Understanding whether these changes did occur is the goal of the JOBSTART evaluation.

It is an open question whether helping young disadvantaged people increase their educational attainment will lead to increased earnings, especially in the short run. Numerous studies have found that people with higher levels of education earn more than do those without a high school diploma. For example, one estimate, based on the experiences of young men and women during the late 1970s, found that the payoff of an additional year of secondary school was approximately $700 in increased annual income, while a high school diploma had a "credential effect" of about $925 per year.[7] But direct rigorous tests of the impact of *increasing* the educational attainment of a group of disadvantaged young people who did not complete high school are rare.

---

[4] Such funds came from Title IIA of JTPA, the largest portion of the JTPA program, or Title IVB, which funds the Job Corps Centers.

[5] The General Educational Development Test is a national examination produced and administered by the GED Testing Service of the American Council on Education in Washington, D.C. States have different criteria for who may take the examination, different passing scores, and different credentials awarded to those passing (for example, a state high school equivalency certificate or a state high school diploma). In accordance with common usage, the credential is referred to in this report as a GED certificate or, simply, a GED.

[6] The program could also affect other aspects of the young people's lives. They might be better able to live on their own instead of with their parents, more likely to postpone childbearing (because they see opportunities in the labor market), and less likely to engage in criminal or other antisocial behavior.

Although, in general, increasing the skills of young people does increase their earning capacity, there are two important countervailing effects of a program like JOBSTART, at least in the short run. Participation in an intensive program "pulls" people out of the labor force. Not only do they give up earnings while they participate in the program, but they also have less chance to gain skills and seniority through their on-the-job experience. For young people, especially those with low levels of educational attainment, work experience is an important source of new skills and increased job stability and wages.[8]

Further, little is known about how employers assess receipt of a GED, the primary educational outcome in "second chance" programs like JOBSTART. The assumption behind such programs is that employers will view a GED as evidence of increased skills, but virtually nothing is known about how long it takes for a GED to pay off and how this might vary among subgroups of youths and types of occupations. Alternatively, and less optimistically, a GED may do little to counter the negative impression created by the fact that the young person did not finish high school.[9]

Earlier reports on the JOBSTART Demonstration, summarized and updated in this document, found that the first four conditions for program impacts listed above were generally met: The programs recruited the target group of youths; they generally offered the intended services; the youths participated in education and training; and this led to increased GED receipt.[10] Thus, the JOBSTART Demonstration presents a unique opportunity to see whether and how enhanced educational attainment and vocational skills training for disadvantaged youths are "processed" by employers in the labor market.[11]

---

[7] Berlin and Sum, 1988. These estimates attempted to control for the level of basic skills of individuals, by including youths' scores on the Armed Forces Qualifying Test, administered as part of a special survey of youths, as an independent variable in a regression. The estimated effect on annual earnings of an additional grade-equivalent of basic skills (for example, progressing from a seventh- to an eighth-grade reading level) was $185.

[8] An unpublished analysis by Andrew Sum, using the *Current Population Survey*, finds that the average earnings of high school dropouts increase with age noticeably during the late teenage years and early and mid 20s. In 1986, for example, the annual average earnings of 19-year-olds were about $2,000 higher than those for 18-year-olds.

[9] A disproportionately higher labor market reward for completing high school than for dropping out part-way through may indicate that employers rely on high schools more for screening workers than for teaching them basic skills (see Layard and Psacharopoulos, 1974). If this is true, attaining a GED after dropping out may add little to a young person's employability.

This report, covering two years in the lives of the young people in the demonstration, is about the JOBSTART program's early effects. As with most investments, JOBSTART had up-front costs. For the young people, the costs were (1) the time and effort devoted to participating in the program, (2) the income forgone by not working, and (3) the lost opportunity to learn new skills on the job. For the public, the cost was financial support for the program activities. As with many other investments, the costs were incurred quickly, while the benefits may accrue over an extended period.

With only two years of follow-up on the experiences of the young people, this is an interim report rather than the final story on JOBSTART. It summarizes the project's policy goals and research design; describes the nature and intensity of the education, training, and other services the young people participated in; and presents 24-month program impacts on educational attainment, employment, earnings, receipt of public assistance, and other outcomes. Throughout, the analysis examines whether the program has worked differently for subgroups of the young population it served.

This chapter discusses aspects of the youth employment problem that shaped the program model and its evaluation. It then examines the policy and programmatic context in which JOBSTART emerged, highlighting how the rules and reality of the JTPA system strongly affected the demonstration guidelines. It also describes how events have made the findings relevant to a larger policy community seeking ways to serve young dropouts, including young mothers receiving public assistance. The chapter ends by summarizing the service guidelines for the program, important differences among the 13 sites, and the topics addressed in the chapters that follow.

---

[10] See Auspos, 1987; Auspos et al., 1989.

[11] Understanding more about the longer-term labor market value of a GED is a central goal of the four-year follow-up to be analyzed in the final report on the JOBSTART Demonstration. That report will examine longer-term program impacts.

I. The Youth Employment Problem and the Challenge of Serving Young School Dropouts

At the heart of the youth employment problem is "a small group of young people who remain out of work a large portion of the time."[12] Overwhelmingly, they are from poor families, have dropped out of school, and lack the basic skills needed to succeed in the job market.[13] Many are members of minority groups, some of whom confront the continuing existence of job discrimination. Young people who have dropped out of school and are unemployed – or only sporadically employed – may face lingering negative consequences.

A. A Closer Look at the Employment Problems of Young School Dropouts

In an economy increasingly reliant on service sector employment, it is virtually impossible for young school dropouts to find stable, well-paying jobs. Numerous national studies have found that dropouts are more likely than other young people to be poor academic achievers who had trouble fitting into a structured environment (the schools) and who lack self-esteem and – often – the habit of persevering in complex and challenging tasks.[14] Employers value the very skills and qualities dropouts often lack: They want employees who have the basic academic skills and a readiness to learn and to set goals, communication skills, self-esteem, and the motivation to follow through.[15] Many black and Hispanic youths face a further obstacle to employment. They are more likely than whites to drop out, but less likely to have access to informal networks that are useful for finding well-paying, steady work.

The resulting employment statistics highlight graphically the circumstances of young school dropouts. Those young people without four years of high school are much less likely to be working and more likely to be out of the labor force or (if in the labor force) unemployed.[16] For example, as Table 1.1 shows, in 1990, 76 percent of all 16- to 24-year-

---

[12] Clark and Summers, 1982, p. 200. See also Ellwood, 1982; Rees, 1986; Hahn and Lerman, 1985, p. 6. Using data from the late 1970s, the Congressional Budget Office estimated that about 10 percent of all youths accounted for 61 percent of all youth unemployment (U.S. Congress, 1982, p. 12).

[13] Nationally, about 15 percent of all people aged 20 to 24 have not completed four years of high school. See U.S. Department of Education, 1988.

[14] The research on which this section is based is summarized in Ekstrom et al., 1986.

[15] American Society for Training and Development, 1988.

[16] The labor force is defined in official statistics as individuals who are employed or actively seeking employment. Unemployment statistics exclude people who are not in the labor force. Thus, many people who are not working are not counted as "unemployed" because they are not actively seeking work. Figures in this section are from U.S. Department of Labor, 1991.

TABLE 1.1

LABOR FORCE STATUS AND EMPLOYMENT RATES FOR 16- TO 24-YEAR-OLDS NOT ENROLLED IN SCHOOL IN 1990

Subgroup	Percent Employed	Percent in Labor Force and Unemployed	Percent Not in Labor Force
All 16- to 24-year-olds	71.2	9.0	19.8
High school graduates with some subsequent education	85.6	5.6	8.8
High school graduates with no subsequent education	75.6	8.7	15.7
School dropouts[a]	50.6	12.6	36.8
White school dropouts	55.3	11.3	33.4
Black school dropouts	29.0	19.7	51.3
Hispanic school dropouts[b]	57.1	9.3	33.6

SOURCE: U.S. Department of Labor, 1991.

NOTES: The labor force is defined as individuals who are employed or actively seeking employment.

Rows may not total 100.0 percent because of rounding.

[a] Those who did not complete four years of high school but received a GED are defined as school dropouts.

[b] Persons of Hispanic origin may be of any race and thus are included in both the white and black school dropout subgroups.

olds with four years of high school and no subsequent schooling were working, while 16 percent were not in the labor force, and 9 percent were in the labor force but were unemployed.[17] Among 16- to 24-year-olds without four years of high school, only 51 percent were working; 37 percent were outside the labor force; and 13 percent were in the labor force and unemployed. In this same age group, only 29 percent of blacks were working, while 51 percent were outside the labor force, and 20 percent were in the labor force and unemployed. Among the same-aged whites and Hispanics, about 55 to 57 percent were working, while 33 to 34 percent were not in the labor force, and about 9 to 11 percent were in the labor force and unemployed.

B. The Different Circumstances of Young Women and Young Men

In many obvious and important ways, the lives of young men and young women differ. This carries through to the experiences of young people who drop out of school. For young men, the last two decades have brought a deterioration in the ability to earn a living, with the decline in earnings being most serious for those with poor skills. For young women, single parenthood – and scant financial resources for handling it – is a common predicament.

During the 1970s and 1980s, young men without a high school diploma experienced a sharp decline in average earnings (after adjustments for inflation).[18] For example, between 1973 and 1986, the average real earnings of 20- to 24-year-old men who had not graduated from high school and were not enrolled in school declined 42 percent. The picture for black men in this group was especially bleak, with their average real earnings dropping 61 percent. During the same period (which included serious recessions), the earnings of male high school graduates declined 28 percent, while the earnings of male college graduates declined only 6 percent.[19]

The incidence of unwed teenage parenthood increased during the last two decades. While the number of births to American teenagers actually declined between 1970 and the late 1980s, the proportion of those births that were outside marriage rose dramatically, from about

---

[17] This employment-to-population ratio is calculated by dividing all those who were employed in 1990 by the total civilian noninstitutional population.

[18] William T. Grant Commission on Work, Family and Citizenship, 1988, pp. 26-27.

[19] This bleak picture for black male school dropouts also appears in labor force statistics of the type cited earlier. Unpublished U.S. Department of Labor data for 1989 show that only 42 percent of 18- to 24-year-old black men without a high school diploma and not enrolled in school were employed, compared to 69 percent of comparable whites and 75 percent of comparable Hispanics.

30 percent (200,000) in 1970 to about 65 percent (300,000) in the late 1980s. Giving birth as a teenager typically coincides with a disruption in a mother's education and greatly increases her labor market difficulties.[20] Unmarried women who become mothers in their teens and do not complete high school are at great risk of long-term welfare dependence and unemployment.[21] In addition, recent evidence indicates that while teenage mothers can often recover partially from the detrimental effects of early childbearing, their children are more likely to suffer long-term social and educational disadvantages than are children born to older women.[22]

C. Research on Effective Programs for Young School Dropouts

At the time the JOBSTART Demonstration began, program designers seeking insights from the previous research on youth programs found few solid success stories on which to base new efforts. Many programs had been tried, but nearly all evaluations found unfavorable results, were inconclusive, or were seriously flawed.[23]

The one influential exception to this pattern was the residential Job Corps, which a study found to be effective in increasing the educational attainment and earnings of young dropouts.[24] The residential Job Corps provides basic skills education, occupational training, life skills instruction, work experience, job placement assistance, health care, counseling, and other support services to youths who live at centers (often outside urban areas) and participate in the program for up to two years.[25] About 80 percent of Job Corps participants have not completed high school. The residential Job Corps, however, could not be offered to all dropouts: It was a relatively expensive program, accessible only to those willing and able to live away from home, requiring development of work experience positions with employers, and clearly not the answer for all disadvantaged youths.

---

[20] In a national survey of high school students, almost one-fourth of all young women who dropped out of school reported that pregnancy was an important factor in their decision. See Ekstrom et al., 1986, for a discussion of this study.

[21] See Ellwood, 1986. In 1989, only 42 percent of 18- to 24-year-old white women without four years of high school and not currently enrolled in school were employed; for blacks, the comparable figure was 22 percent and for Hispanics, 32 percent. The percentages for young school dropouts who were also mothers would be even lower, but were not calculated owing to sample size limitations in the *Current Population Survey*.

[22] See Furstenberg, Brooks-Gunn, and Morgan, 1987.

[23] A common methodological problem was the absence of an appropriate group (one that was similar to participants but not served by the program) against which the experiences of the group that was served could be compared. Without such a comparison, evaluators frequently confused outcomes that followed a program with the real difference a program made – in the language of evaluation, its "impacts." For example, the *outcomes* of a program might include a post-program job placement rate of 50 percent. However, the employment rate for the appropriate comparison group might also be 50 percent, suggesting that the program had no *impact* on employment rates. See Betsey et al., 1985, and the discussion in Chapter 2 of this report for more on this issue.

[24] See Mallar et al., 1982.

Other efforts to directly connect young people with work – by either helping them look for work more effectively or providing subsidized work experience – were tested in demonstrations in the early 1980s. The evaluation of job search assistance for youths found that the program produced short-term increases in employment and earnings, but that in the long run participants were no better off than a comparison group.[26]

As for the most common youth employment strategy – subsidized work experience – two evaluations failed to find any long-term impacts on educational attainment, employment, or earnings for dropouts. The National Supported Work Demonstration, managed by MDRC in the late 1970s, enrolled very disadvantaged young dropouts (many with a criminal record) in a 12- to 18-month program of paid work experience with gradually increasing job responsibilities. Program impacts for this group were not positive, even though the program proved successful for long-term welfare recipients.[27] The Youth Incentive Entitlement Pilot Projects (YIEPP), which offered subsidized minimum-wage jobs to high school students and dropouts who returned to school, also was ineffective for dropouts.[28] While the program did increase the employment and earnings of young people still in school, evaluators found that the offer did not induce dropouts to return to and remain in regular high school. Many of those who did return dropped out a second time, and there were no effects on educational attainment, employment, or earnings for dropouts.

---

[25] Some Job Corps Centers also operate a nonresidential program. These were not included in this earlier study. As previously noted, three nonresidential Job Corps programs were included in the JOBSTART Demonstration.

[26] The demonstration assessed the effectiveness of a program providing job search assistance through simulated interviews, seminars on job-seeking techniques, and help in making contact with potential employers. See Public/Private Ventures, 1983. This finding differs from that of research on job search assistance programs for women receiving Aid to Families with Dependent Children (AFDC), which did find long-term employment and earnings impacts. See Gueron and Pauly, 1991.

[27] See Maynard, 1980, on the findings for young dropouts and Manpower Demonstration Research Corporation, 1980, for the results for AFDC recipients.

[28] See Gueron, 1984.

Thus, the research record of the mid 1980s put the Job Corps in a special category as an effective program for raising the employment and earnings of young school dropouts.[29] As would be expected in an intensive program of skills enhancement, Job Corps participants initially earned less than their comparison group counterparts.[30] This pattern of earnings continued for a time after people left the program because those in the comparison group had accrued more work experience (an important source of skills-building), seniority and protection against layoffs, and promotions.[31] The positive effects of enhanced human capital did not outweigh the negative effects of lost work experience until roughly 6 to 12 months *after* participation in the program, the first semi-annual period in which the average earnings of program participants exceeded those of their comparison group counterparts.

Not only did the research record find positive impacts for the residential Job Corps program, but it also found program benefits to be greater than program costs. Although program costs per participant were much higher than for most other programs (averaging about $5,000 in 1977), the estimated benefits to society exceeded the costs. Especially encouraging was the program's positive findings for young male dropouts, a group that had proven especially hard to serve in many previous programs.

Among the questions left open by the existing research, however, was whether the Job Corps approach could be successfully adapted to a new setting: operation as a nonresidential program by other agencies, which could not offer comprehensive support services and probably would not have the Job Corps' level of resources to devote to staff training, facilities, and curriculum. This shift to a nonresidential program is an important one, since some of the benefits of the traditional Job Corps program seem to stem from its residential nature. For example, in a residential program, it is much easier to provide an intensive program of support services (including counseling outside class time, positive peer support, recreational activities, and health care) than when young people are active in the program for at most eight hours a day. Furthermore, the decline in criminal activity and substance abuse observed for Job

---

[29] The National Academy of Sciences, in its review of research on employment programs for young people, pointed out the distinction between the failure of research to provide adequate evidence of program effectiveness and the finding that a program is ineffective. Betsey et al., 1985.

[30] This would occur because of participants' forgone earnings and lost opportunities for on-the-job skills enhancement while they were in the program, as discussed earlier in this chapter.

[31] Evaluations of the Job Corps discussed the problem of the post-program transition back into the labor market that led to these initial negative impacts. See Mallar et al., 1978, 1980.

Corps participants (especially during program participation) was partly attributable to their isolation in residential centers outside urban areas, or at least outside their previous neighborhood. While the residential nature of the program may have been a factor in its success, it did pose problems for some young people. For young mothers with child care responsibilities, the program demanded too much time away from home, and it did not prove effective for them. Also, many young men and women did not wish to leave their communities.

In 1983, the National Academy of Sciences convened a panel of experts on youth programs. Their assessment – summing up research findings – recommended further testing of the Job Corps approach in a nonresidential setting using random assignment to produce the most reliable findings.[32] The JOBSTART Demonstration was, in part, a response to this call.[33]

D. Growing Operational Experience in Programs for Young Dropouts

Program operators serving young, disadvantaged dropouts have identified a number of lessons that also informed the development of the JOBSTART Demonstration.[34] When serving these youths, who often look back on past educational experiences with dissatisfaction, programs have to actively seek out participants rather than passively wait for volunteers to come forward. Program operators have also learned that achieving continued participation is not easy: Counseling and peer support have often proved useful in improving young people's self-esteem and motivation, but even with these efforts, participation levels can be disappointing.

Finally, program operators have increasingly become sensitive to the multiple needs of clients. For some economically disadvantaged young people, a low level of basic skills prevents them from taking advantage of occupational training. In addition, as is the case for many young people, economically disadvantaged youths may not be experienced in setting goals, making plans to achieve them, and following through with effective action. And finally, many young people have a pressing need for immediate income, for themselves or their families, so programs must help them find a means of financial support while they invest in their future by enhancing their skills.

---

[32] Random assignment is generally recognized to be a reliable method of measuring the effectiveness of new programs of employment and training. As discussed later in this report, it was used in the JOBSTART Demonstration.

[33] As the later discussion of the JOBSTART program model and its implementation will make clear, JOBSTART did not offer the same comprehensive list of support services available in the Job Corps. Nor did it use the same curriculum in education or training, except in the three sites that already operated a Job Corps nonresidential program. Nationally, about 10 percent of Job Corps participants are in nonresidential programs.

[34] Many of these lessons are summarized in 70001 Training and Employment Institute, 1988, and Public/Private Ventures, 1990.

II. The Changing Programmatic Context of the JOBSTART Demonstration

Because the JOBSTART Demonstration did not have special program funding to support site operations, it was shaped in important ways by the need to find a funder for local JOBSTART sites within existing programs. Since JTPA was the most likely source of local operational funds, its provisions and the local interpretation of them were central to the structure of the project. As the demonstration unfolded, JTPA changed in ways that make the JOBSTART findings even more relevant to the JTPA system. And a new audience for the demonstration findings has emerged with the passage of the Family Support Act of 1988, calling for expanded programs of education and employment services for young women who are school dropouts and receive welfare.

A. JTPA as a Crucial Funder of Program Services

The Job Training Partnership Act (JTPA) of 1982 is the federal government's major program for funding employment and training for economically disadvantaged adults and youths. JTPA distributes the majority of its funds to states which, in turn, pass along most of what they get to local administrative entities called service delivery areas (SDAs).[35] The federal JTPA statute sets general rules for program eligibility and allowable types of activities. An SDA's staff and private industry council (PIC) – often operating like a board of directors for the agency – determine what specific types of services are to be offered, which groups will get priority for services, and how service providers under contract to the SDA are to be evaluated and paid.

The manner in which JTPA was initially implemented during the mid 1980s presented operational constraints that had to be taken account of in the design and implementation of the JOBSTART Demonstration:

---

[35] Most JTPA funds under Title IIA, the largest part of JTPA, are distributed to states, using a formula based on the states' number of unemployed and economically disadvantaged people.

- **Performance standards that made SDAs hesitant to serve youths with very poor skills.** The incentives embedded in Title IIA, the largest part of JTPA and the one that finances most youth programs, made SDAs and JTPA-funded education and training agencies hesitant to enroll youths with very poor basic skills who were in need of intensive programs of education and training and support services. In designing and applying the performance measures used during the first five years of JTPA, federal, state, and local administrators focused on the proportion of participants placed in a job, their wages, and the cost per "success story."[36] This encouraged SDAs and service providers to choose people who were more likely to achieve these successes at relatively modest costs.[37] In seeking to serve school dropouts with poor skills in an intensive program, JOBSTART had to confront these incentives.

- **Performance contracts that inhibited combined education and training.** Many SDAs wrote contracts with service providers that linked payment to the achievement of the events measured in performance standards. This made it difficult for those service providers that wished to serve youths with poor skills to be paid and complicated the administration of program models that required both education and training when a single agency could not provide both.

- **Severe restrictions on paid work experience.** Experience with public service employment under JTPA's predecessor, the Comprehensive Employment and Training Act (CETA), led Congress to eliminate most forms of paid work experience when enacting JTPA.

- **Tight limitations on support services.** The statute limited spending on support services (such as transportation and child care assistance) and needs-based cash payments and completely eliminated the payment of stipends to participants.

---

[36] From the early 1980s until program year 1987 (ending in June 1988), the performance of SDAs serving adults was judged by the following standards: the percentage of adults who found a job; the percentage of adults who were receiving welfare when they enrolled in JTPA and who found a job; the average wage at placement in a job; and the program cost per person entering employment. For youths, the standards included the percentage who found employment and the "positive termination rate," defined as entering employment or other quantifiable measures of program success. These included attainment of employment competencies recognized by local private industry councils, completion of a level of schooling, enrollment in further non-Title IIA training, enlistment in the armed forces, return to school full time, or (for 14- and 15-year-olds) completion of specified program objectives. The youth standards included the cost per "positive termination." For each measure, the U.S. Department of Labor set national levels, which – at state option – could be adjusted to reflect the characteristics of those served and the conditions in the local labor market.

[37] Data from the mid 1980s illustrate the effects of these program priorities. During program years 1984 to 1986, when the JOBSTART Demonstration was beginning, young dropouts constituted only 11 percent of all Title IIA participants and 27 percent of all young participants. Among young dropouts who were served under Title IIA nationally in 1986, only 23 percent received basic education, a service likely to promote their long-term employability but unlikely to lead to immediate placement in a job.

The early experience under JTPA prompted allegations that the program was making little real difference in participants' lives because service providers tended to enroll more job-ready applicants (a practice known as "creaming").[38] Further, sharp declines in the unemployment rate during the 1980s, which allowed many more job-ready individuals to find work, caused a rethinking about whether JTPA should continue to emphasize quick placement of participants in a job. Over time, Congress, the U.S. Department of Labor, and program operators have all expressed renewed interest in intensive programs of education and training targeted on more disadvantaged youths.

Responding to the early pattern of program operation, the U.S. Department of Labor changed its administrative practices and regulations and encouraged greater provision of intensive services for youths through a formal demonstration of such programs. In late 1987, the Department stated that "more emphasis must be placed on intensive investments in youth within JTPA" and recommended that "a significant portion of youths who participate . . . should receive competency-based instruction in either basic education or occupational skills."[39] Soon thereafter, amendments to the regulations (effective in program year 1988) encouraged states to choose as the key standard for youth programs one that includes measures of increased educational and skills competencies. This increased the opportunities to include young dropouts with poor skills in JTPA. In addition, the Department in 1989 issued a request for proposals for the Youth Opportunities Unlimited (YOU) Demonstration and in 1990 selected seven sites to operate innovative programs.[40]

---

[38] The U.S. Department of Labor has contracted for an independent study of JTPA to determine program impacts in a sample of 16 SDAs across the country. The study is being conducted by Abt Associates Inc., MDRC, NORC, and Lewin-ICF. See Doolittle and Traeger, 1990. The first impact results will be available in early 1992.

[39] *Federal Register*, December 16, 1987.

[40] Sites applying to participate were required to operate one of three programs: a work experience program modeled on Ventures in Community Improvement (VICI), which operated from 1978 to 1980; an alternative high school program modeled on High School Redirection in Brooklyn, New York; and a program of education and training modeled after JOBSTART. Seven sites were chosen for the three-year demonstration; some of them chose to operate a program modeled after JOBSTART. An implementation study is part of that demonstration, which is separate from the JOBSTART Demonstration reported on here.

Debate over further changes in JTPA also signaled greater interest in more intensive programs for young dropouts. An advisory committee to the U.S. Department of Labor recommended shifting more JTPA resources to harder-to-serve youths and ending restrictions on the support services these youths are likely to need.[41] Although JTPA was not amended in the 1989-90 session of Congress, nearly all of the proposals would have abolished or downplayed cost standards for youth programs and required greater targeting of program resources on youths with multiple barriers to employment, a group likely to need more intensive services.

While this interest in hard-to-serve youths came too late to affect the implementation of the demonstration, it has heightened the importance of the project as an early test of a new direction for JTPA and has increased the chances that the JOBSTART program will be successfully replicated if the research findings are positive.

B. The Family Support Act

Passage of the Family Support Act of 1988 (FSA) signaled a growing emphasis on programs of education and employment services for AFDC recipients, making the JOBSTART findings relevant for organizations serving this group. The Job Opportunities and Basic Skills Training (JOBS) title of the Act expands the obligations of AFDC mothers to participate in activities intended to increase their employability, and of states and counties administering the AFDC program to offer more education and training than were typically offered under the predecessor Work Incentive (WIN) program. Especially relevant to JOBSTART is the fact that the JOBS legislation allows states to impose a participation obligation on AFDC parents under 20 years of age who lack a high school diploma or GED regardless of the age of their child.[42] For this group, education is normally presumed to be the appropriate first activity. States and local service providers seeking to expand their offerings of education and training for young mothers are currently grappling with many of the issues addressed in JOBSTART.

---

[41] Job Training Partnership Act Advisory Panel, 1989.

[42] While single parents with children under age three are normally exempt from participation in JOBS programs, this is not the case for custodial parents under age 20 who have not graduated from high school or received a GED.

III. The JOBSTART Demonstration

MDRC began the JOBSTART Demonstration in 1985 with two purposes: (1) to determine the operational feasibility within JTPA of an intensive program incorporating several of the key elements of the residential Job Corps, and (2) to rigorously test its effectiveness.[43] Local and state JTPA agencies provided most of the operational funding for the JOBSTART sites, but the MDRC evaluation was funded by an unusual consortium consisting of the U.S. Department of Labor, the Rockefeller Foundation, the Ford Foundation, Charles Stewart Mott Foundation, the William and Flora Hewlett Foundation, the National Commission for Employment Policy, AT&T Foundation, Exxon Corporation, ARCO Foundation, Ætna Foundation, the Chase Manhattan Bank, and Stuart Foundations. Funding from this consortium also enabled MDRC to award a modest $25,000 grant to each site.

This funding structure shaped the character of the demonstration at the local level in two important ways. First, the JOBSTART program operated within existing agencies and programs under the rules and performance standards of Title IIA of JTPA or, for the nonresidential Job Corps Centers, under Title IVB of JTPA. It proved a serious challenge for the non-Job Corps sites simultaneously to follow the demonstration guidelines, the rules of Title IIA of JTPA, and the provisions in their contracts with SDAs.

Second, without special funding, sites could not be expected to make major changes in their existing programs, limiting the extent to which the JOBSTART curriculum and instructional methods could be standardized. Instead, MDRC gave sites general guidelines for program operation specifying the type and duration of required components of the program (education, occupational training, job placement, and support services). Even within this flexible framework, some program operators faced major implementation challenges. Some of the sites normally offered only basic skills education or vocational training; the demonstration called for both, requiring them either to add a whole new kind of activity or to link up with other local agencies providing it. Some sites also had to adapt to a younger and less skilled student body than they normally served. The lack of special program funding also limited the extent to which non-Job Corps sites could offer the array of support services that were a part of the Job Corps program.

---

[43] See Auspos, 1987, for a discussion of the origins of JOBSTART and its early implementation.

The demonstration was thus a hybrid: part evaluation of existing programs and part test of a new program. The basic program differed from site to site in myriad details, but the variety did permit a test of how a scaled-down Job Corps-type program could operate under existing rules in different kinds of established agencies. If the demonstration showed positive results, it would be easier to replicate the program widely.

A. The JOBSTART Sites

MDRC staff recruited sites each of which they thought could (1) meet the JOBSTART program guidelines (discussed below) with little or no technical assistance except on techniques of client outreach and retention, (2) assemble sufficient operational funding for the full array of JOBSTART services (a significant barrier, as discussed above), and (3) yield a target of 200 sample members. A total of 13 sites across the country, listed in Table 1.2, participated in the demonstration.[44] All had experience running programs that included some or all of the components of the JOBSTART model or working with young dropouts. As noted earlier, most of the operating funds for the demonstration sites were provided through the regular JTPA system under Title IIA of the legislation. (The Job Corps, as also noted earlier, is separately funded and administered under Title IVB of JTPA, so the three Job Corps Centers in JOBSTART received funding through that title.)

While all agreed to implement the JOBSTART model, the sites brought to the demonstration varying operating experiences:

- **Sponsoring organizations.** The participating organizations included adult vocational schools, a community college, community-based organizations that focus on literacy development and GED preparation, community-based organizations that focus on occupational skills training, and the nonresidential components of three Job Corps Centers.[45]

- **Prior service emphasis.** Some sites previously had offered only basic education and no skills training, while others had offered both but had emphasized skills training. The education-focused sites may have attracted youths who were primarily interested in basic education rather than skills training. Similarly, some sites with strong histories of skills training may have attracted youths who were primarily interested in learning the skills needed for a particular occupation rather than attaining a GED.

---

[44] See Auspos, 1987, and Auspos et al., 1989, for a detailed discussion of the characteristics of the sites in the demonstration.

[45] The Job Corps Centers operated their usual nonresidential programs. Thus, they offered all JOBSTART services plus other Job Corps services that are not part of the JOBSTART model.

TABLE 1.2

THE JOBSTART SITES

Agency Name	Location	Type of Organization	Prior Service Emphasis[a]	JOBSTART Program Structure[b]
Allentown Youth Services Consortium[c]	Buffalo, NY	Community-based	Education	Sequential/brokered
Atlanta Job Corps	Atlanta, GA	Job Corps Center	Education and training	Concurrent
Basic Skills Academy (BSA)	New York, NY	Community-based	Education	Sequential/brokered
Capitol Region Education Council (CREC)	Hartford, CT	Community-based	Education	Sequential/brokered
Center for Employment Training (CET)	San Jose, CA	Community-based	Training with some education	Concurrent
Chicago Commons Association's Industrial and Business Training Programs	Chicago, IL	Community-based	Training	Concurrent
Connelley Skill Learning Center	Pittsburgh, PA	Adult vocational school	Education and training	Concurrent
East Los Angeles Skills Center	Monterey Park, CA	Adult vocational school	Education and training	Concurrent
El Centro Community College Job Training Center[d]	Dallas, TX	Community college	Education and training	Sequential/in-house
Emily Griffith Opportunity School (EGOS)	Denver, CO	Adult vocational school	Education and training	Concurrent
Los Angeles Job Corps	Los Angeles, CA	Job Corps Center	Education and training	Sequential/in-house
Phoenix Job Corps	Phoenix, AZ	Job Corps Center	Education and training	Concurrent
SER/Jobs for Progress	Corpus Christi, TX	Community-based	Training	Concurrent

(continued)

TABLE 1.2 (continued)

NOTES: [a] "Education" refers to basic education, often as preparation for the GED examination. "Training" refers to instruction in occupational skills needed for specific jobs.

[b] *Concurrent* programs offer basic education and occupational training concurrently from the beginning of participation. *Sequential/in-house* programs offer basic education followed by occupational training, with both components provided in-house by the agency. *Sequential/brokered* programs provide basic education and then serve as a broker for occupational training, referring participants to other agencies.

[c] In October 1990 this site was renamed The Clarkson Center, Inc.

[d] In September 1988 this site was renamed the Edmund J. Kahn Job Training Center.

This diverse background led the sites to implement the basic JOBSTART program components in several ways. Eight sites were able to offer both education and training in-house and chose to provide them concurrently, with participants active in both activities from the start. Two sites provided both activities in-house, but offered them in sequence, with skills training following education. The remaining three sites did not have the capacity to offer skills training and chose to provide basic education themselves and work with other agencies to place their participants in subsequent occupational training elsewhere.

B. The JOBSTART Program Guidelines

Drawing on the lessons of the Job Corps and applying them within the constraints of JTPA, the demonstration developed a new alternative program offered in a nonresidential setting with fewer support services available to participants. The key elements, shown in Table 1.3, included the core components of the Job Corps (basic education, occupational training, and job search) but a less extensive system of support services.[46] In some respects (the definition of the target population and the requirement that certain activities be included), the program model was quite specific, while in others it allowed for considerable variation. The model set requirements as to the type and intensity of education and training services that were to be offered to participants, and it placed strong emphasis on the need for strategies to increase program retention. However, as mentioned earlier, sites were given a great deal of flexibility in implementing these core requirements.

1. Target group. Since the program was designed to reach a population largely unserved by existing programs, eligibility requirements were quite specific. Participation was limited to school dropouts who were between 17 and 21 years of age, did not have a high school diploma or GED, read below the eighth-grade level, and satisfied the JTPA definition of economically disadvantaged (defined primarily by household income or receipt of public assistance). Recognizing that program operators needed to meet enrollment and performance standard targets, however, the guidelines allowed for up to 20 percent of participants to read at or above the eighth-grade level.

---

[46] Chapter 3 of this report provides more detail on the JOBSTART program model as implemented by the sites in the demonstration. A fuller description is given in Auspos et al., 1989.

TABLE 1.3

THE JOBSTART PROGRAM GUIDELINES

Target Population	To be eligible for JOBSTART, individuals had to be: · 17 to 21 years old · school dropouts without a high school diploma or GED · reading below the eighth-grade level on a standardized test[a] · economically disadvantaged[b]
Basic Education	Sites were to implement a curriculum that: · was self-paced and competency-based · was computer-managed and -assisted, if possible · was a minimum of 200 hours in length · focused on reading, communication, and basic computation skills
Occupational Skills Training	Sites were to implement a curriculum that: · was in a classroom setting · combined theory and hands-on experience · prepared enrollees for jobs in high-demand occupations · provided at least 500 hours of training · had been developed with the assistance of the private sector to ensure that graduates would meet the entry-level requirements of local employers
Training-Related Support Services	Services were to be tailored to individual needs and were to include, in addition to transportation and child care, some combination of the following: · work-readiness and life skills training · personal and vocational counseling, mentoring, tutorial assistance, and referral to external support systems · needs-based payments or incentive payments tied to length of stay, program attendance, or performance
Job Development and Placement Assistance	JOBSTART operators and/or their subcontractors were to be responsible for assisting participants in finding training-related jobs

SOURCE: Manpower Demonstration Research Corporation, 1985.

NOTES: [a] To help meet enrollment targets, each site was allowed to enroll individuals - up to 20 percent of its total JOBSTART enrollment - who read at or above the eighth-grade level.

[b] To be eligible for JTPA services - economically disadvantaged by JOBSTART standards - a person must be receiving public assistance; have family income at or below the poverty line or 70 percent of the lowest living standard income level; be homeless, under the definition of federal statutes; or, in some cases, be a handicapped adult whose own income fits within the guidelines but whose family income exceeds it.

2. **Education and training.** The demonstration sought to test an intervention that would be relatively intensive and lengthy compared to the usual JTPA activities and that would address the multiple deficits in participants' skills. As a result, the program model required sites to offer a specified minimum amount of both basic education and occupational training to provide the young people with a real opportunity to enhance their skills. This combination of services, as noted earlier, differed from the usual situation under Title IIA of JTPA. The 200-hour minimum of education was based on an estimate of what would be needed to bring the basic skills of most participants reading below the eighth-grade level up to the point where they could qualify for a GED or enter occupational skills training. The 500 hours of training was a compromise between the very lengthy training that research suggested was useful and what was practical in most JTPA environments. Given the difficulty of keeping young people engaged in a program for an extended period and the competing demands on their time (including their need for income and their child care responsibilities), staff recognized that not all participants would complete these activities and that the total time in the program would be a year or less.

The two instructional components were structured in ways intended to make them appealing and accessible to young people who entered the program with widely varying levels of skills. For basic education, the guidelines required sites to offer instruction in reading, communication, and basic computational skills, using individualized curricula that allowed youths to proceed at their own pace toward required competency goals.[47] The program model did not specify any particular curricula, though it did encourage – but not require – sites to offer computer-assisted instruction.

The occupational skills component required classroom rather than on-the-job training, in the belief that participants would benefit from the intensive, closely supervised instruction possible in a classroom setting. Again, no specific curriculum was required. Recognizing the advantages of applying learning to practical problems, however, the program model required that the training include a combination of theory and hands-on experience. Seeking to increase the chances of placement following training, the program model required that the training prepare participants for jobs in high-demand occupations and be developed in cooperation with local representatives of the private sector.

---

[47] Sites were also expected to supplement this individualized instruction with group activities.

3. Support services. Attracting and keeping disadvantaged youths in education and training programs is a challenging problem, and the sites were expected to assist participants with transportation and child care. They were also encouraged to develop a package of other support services to facilitate program participation; the Job Corps sites offered considerably more support services than did the others.[48]

4. Job placement assistance. The guidelines required sites to identify possible training-related jobs for participants and to assist them in securing employment, but were not specific about how this should be done. All sites instructed the youths on work disciplines, employer expectations, and job search techniques, but the intensity of this effort ranged from informal guidance by counselors and other staff to more than 50 class hours in one site. Seven sites offered some form of work experience or internships (both paid and unpaid) to improve job skills. All sites provided assistance in seeking employment when the youths left the program, although in two of the three sequential/brokered sites (CREC in Hartford and BSA in New York City), the responsibility fell solely on the training provider. This arrangement for job search assistance proved a serious limitation since, as will be discussed in Chapter 3, many young people did not reach the training phase in sequential/brokered sites.

C. Key Dimensions of Local Variation

The previous report on the implementation of JOBSTART highlighted two dimensions of local variation as important influences on the program experience of the JOBSTART youths:

- **Concurrent versus sequential education and training.** Programs could offer youths basic education classes and vocational skills instruction at the same time (a concurrent model) or basic education before skills training (a sequential model).

- **In-house versus brokered services.** Programs could offer youths education and training at the same agency, or the agency providing basic education could serve as a broker, helping participants who were completing the education phase to find appropriate training at other institutions (sequential/brokered sites).

---

[48] Job Corps Centers offered health services, recreational activities, and on-site food service, and more intensive counseling and peer support than did most other sites.

In the period following the last JOBSTART report, research on other programs has also called attention to variations among sites offering concurrent programs. The Minority Female Single Parent (MFSP) Demonstration, funded by the Rockefeller Foundation, tested different models of education and training in four local agencies.[49] The early evaluation reports based on one year of follow-up argue that the one program among the four in the study with positive impacts on employment and earnings (CET, operating in the San Francisco Bay area) achieved this result because of programmatic and organizational features that distinguished it from the remaining three.[50] Specifically, researchers highlighted "the training design of the CET program – which emphasized training for all regardless of educational skill levels, offered remedial education within the context of job skill training, and accommodated trainees with diverse levels of educational skills."[51] The hallmark of such an integrated program is a focus on vocational training, aided by the teaching of literacy and mathematics skills needed in the chosen occupational area (rather than general literacy and mathematics training). The U.S. armed forces have experimented with this approach in teaching military occupations to recruits with poor basic skills.[52]

In practice, there is no clear distinction between integrated concurrent programs and other concurrent programs; they form a spectrum rather than falling into two neat categories. Among the JOBSTART sites, for example, CET/San Jose operated the most integrated program. Basic skills instruction was largely incorporated into the training curriculum, with the goal of supporting the learning of occupational skills. The challenge for this approach is to serve participants with very low levels of basic skills. Some people in this group will initially be unable to participate in serious vocational training, so program operators face a choice of offering them special help in literacy and mathematics or routing them to less demanding training courses. This trade-off is illustrated by the experiences of a second site, which operated a partially integrated program. Chicago Commons offered several training courses requiring technical knowledge and mathematics skills. Even after imposing entrance requirements among the most stringent in any JOBSTART site, Chicago Commons found that the skills deficiencies of some participants were too severe to be addressed within the integrated training context. Thus, the site also offered a separate basic education class. Other concurrent sites such as SER/Corpus Christi and Connelley in Pittsburgh operated separate education classes aimed at preparing people to pass the GED examination. These sites tried to coordinate the activities in education and training classes via conferences among the instructors and inclusion of basic skills instruction in some training classes.

---

[49] See Gordon and Burghardt, 1990.

[50] The CET site in the MFSP Demonstration enrolled minority female single parents, whose average age was 28, and served them in San Jose and several other East Bay communities. The San Jose program was also a site in JOBSTART, but only 10 mothers became part of the JOBSTART sample at CET/San Jose, and they were considerably younger than the MFSP sample.

[51] Gordon and Burghardt, 1990, p. xxvi. The authors also cited a number of other factors that were unique to CET among the four sites and that they believe contributed to its large impacts, including its financial stability and experienced staff; integration of the MFSP program into an ongoing training operation with a similar mission; large scale, which allowed for training in a variety of occupations in demand in the local economy; attention paid to job placement; and availability of on-site child care. The remaining three sites in the MFSP Demonstration emphasized "the acquisition of basic skills before entry into job skill training" (p. xxvi) – that is, sequential programs in the terminology used in the present report.

[52] This approach is sometimes call "functional context training." See Sticht, 1987.

The strong policy interest in these issues has led to a lively debate about the arguments for and against each approach.

1. Sequential versus concurrent versus integrated education and training. Proponents of sequential (as opposed to concurrent) programs argue that youths who are reading and computing at low levels get more out of training if their basic skills are improved before they enter occupational coursework.[53] The youths, they maintain, will have more choices of training and can get more out of the instruction.[54] Sequential programming also eases the burden of scheduling classes since students are freed from the pressure of simultaneously participating in two types of intensive coursework. Furthermore, their daily schedule can allow time for activities designed to address a variety of needs, such as life skills training, recreational activities, or part-time jobs.

Advocates of "nonintegrated" programs (both sequential and concurrent) believe that employers value workers with a broad range of basic skills, not just those needed to master a specific occupation. In this view, workers with these more general skills will learn new skills more quickly on the job, be more flexible and productive than other workers in the long run, and be able more easily to shift to a new employer or even a new occupation when the economy changes and their original job no longer exists.[55]

---

[53] Hahn and Lerman, 1985.

[54] Many existing occupational curricula require that entrants have tenth-grade reading and mathematics skills.

[55] See, for example, National Academy of Sciences, 1984; Johnson and Packer, 1987; National Association of Manufacturers, 1982.

Advocates of concurrent – and especially integrated – programs focus on a different issue: the difficulty of keeping disadvantaged young school dropouts in programs. Students may find the education phase of a sequential program too much like their past high school experience, which many did not like, and may leave the program before they get to occupational skills training. In this view, being able to combine basic education with skills training – which has a more obvious connection to the job market – makes the education component more appealing.[56] It is argued, for example, that if students see that they need basic mathematics in order to make measurements for carpentry, they will be more motivated to solve addition and division problems.

Another argument for concurrent programming – and especially integrated programs – rests on a narrower view of the purpose of basic skills education for young dropouts. Its proponents hold that instruction in basic skills should focus on the particular skills needed for a job (the goal of most young people in the program) rather than on imparting general knowledge that is less directly relevant to the lives of the youths.[57]

2. Brokered versus in-house services. Practically speaking, brokering may be the only way that small agencies specializing in one type of service can provide multi-component, comprehensive programs. None of the small, community-based education providers participating in the demonstration, for example, had the capability to develop on-site training facilities offering a variety of training options. Agencies with a limited number of training courses might also choose to broker training for some participants in order to increase the range or quality of training available to them.

Brokered programs increase the operational challenges for the program operator, however. There are potential difficulties, for example, in ensuring that participants in education will be accepted for training by other agencies, in scheduling the end of the education phase to coincide with a variety of different training schedules, and in monitoring the progress of students referred to other agencies and the quality of the services provided to them.

---

[56] Mathematica Policy Research, 1988.

[57] A final argument for concurrent programs rests on the particular features of most JTPA programs. Programs offering concurrent education and training tend to be shorter and do not involve a transition from education to training. If this transition also involves a shift from an education service provider to a second provider (for training), JTPA performance contracts may typically not allow final payment of the first provider unless participants receive a GED certificate, a milestone recognized under the JTPA performance standards. Since many youths with poor skills will not pass the GED test, this is a practical obstacle to sequential/brokered programs.

3. **A further key dimension of local variation.** JOBSTART sites operated in very different labor markets. The unemployment rates in the sites' metropolitan areas varied from a low of 3 percent in 1987 in Hartford, where CREC is located, to 12 percent in 1986 in Corpus Christi, where SER operated. Youth unemployment rates varied from 6 percent in 1986 in Hartford to 27 percent in 1985 in New York City, where BSA was located.[58]

Later chapters of this report present a more in-depth look at JOBSTART implementation and present generalizations about how local variation affected the way the program operated. However, as discussed in more detail in Chapter 2, caution must be exercised in making cross-site comparisons of program implementation and impacts. Since many features of programs differ among the sites, it is very difficult to isolate the influence of a difference in one factor.[59]

IV. The JOBSTART Evaluation and the Organization of This Report

The evaluation of JOBSTART is divided into three main parts. The first dealt with the sites' implementation of the program. *Launching JOBSTART*, the initial report on the demonstration, discussed site selection and characteristics, the operation of the program within JTPA, and early implementation experiences.[60] A second report, *Implementing JOBSTART*, completed the implementation analysis by describing the content of JOBSTART activities, the participation patterns of the young people in the program, and operational lessons to be drawn from the demonstration.[61] These findings are summarized and updated in this report.

The second part of the evaluation is an analysis of program impacts; the early findings on impacts form the core of this report. The research was designed to separate out the effects of JOBSTART itself from events attributable to other factors (such as other services participants were receiving and events in their lives outside the program). To accomplish this, all people who applied for JOBSTART and were found to be eligible were randomly assigned to either an experimental or a control group. Those in the experimental group were given access to the JOBSTART program services; those in the control group were not, although they could receive other services offered in their community. Since the youths were assigned at random to the two groups, they were similar except for the fact that only the experimental group could receive JOBSTART services.

---

[58] U.S. Department of Labor, Bureau of Labor Statistics, unpublished figures.

[59] The previous report (Auspos et al., 1989) also highlighted variations among the sites: whether they were serving JOBSTART youths in mainstream adult classes or in separate classes for youths; whether they offered computer-assisted instruction; and how they differed in scheduling (that is, the number of hours a day devoted to various activities) and in the length of their courses. Since these did not appear to have a major influence on program implementation or on participation by the youths, they were unlikely to have affected impacts and are not emphasized in this report.

[60] Auspos, 1987.

[61] Auspos et al., 1989.

Individuals in both groups were scheduled to be surveyed 12, 24, and approximately 48 months after being randomly assigned to their group. (The time frame for applying to JOBSTART varied from site to site but ranged overall from August 1985 through November 1987. Hence the fielding of each wave of the survey also extended over many months.) Using these surveys, the experiences of the two groups can be compared to estimate the effect of the program on educational attainment, employment, earnings, use of public benefits, and other outcomes.

The third part of the evaluation will assess the cost-effectiveness of the program. While the present report includes a discussion of the costs of JOBSTART, the final report, based on four years of follow-up, will summarize the findings on program costs and benefits.

As discussed above, this research design uses random assignment to provide reliable information on the central question – the impact of JOBSTART services – by comparing the experiences of two groups that were equivalent except for the experimental group's access to the program. It is important to understand that it was not designed to answer certain other questions with equal rigor. Most important, the study was not designed to rigorously compare the effectiveness of sequential versus concurrent or integrated programs. The young people were not randomly assigned to these different programmatic approaches. Instead, the sites offering these three approaches operated in different kinds of settings and local labor markets and served different types of youths with varying backgrounds and interests. Thus, the structure of the program was not the only difference among sites. Given these many differences, it is impossible to isolate the influence of one factor – such as concurrent versus sequential program structure – on program effectiveness.[62]

---

[62] To rigorously compare the impacts of different programmatic approaches, more than one approach would have to be operated in each site and youths would have to be randomly assigned to one of them. Even with this design, if the programs differed on several dimensions, it would still be impossible to isolate the effect of any one dimension. This type of research has rarely been undertaken. Examples (continued...)

Chapter 2 of this interim report on JOBSTART presents the key research questions and the research design used to address them. It also includes information showing that the sites did succeed in recruiting the young, disadvantaged target group of the demonstration. Chapter 3 discusses the implementation of the JOBSTART program, concluding that most youths in JOBSTART participated in education and training activities more than the typical participant in JTPA-funded programs and nearly as much as the typical Job Corpsmember. Here, as in all subsequent chapters, the report seeks to understand the overall results by examining whether and how JOBSTART operated differently for key subgroups of youths in the sample, with a particular focus on young men and young mothers. Chapter 4 examines educational outcomes, especially the degree of JOBSTART youths' participation in education and training compared to that of the control group, and whether JOBSTART led to increased attainment of a high school diploma or GED during the two years of follow-up. Chapter 5 examines the early indications of how this investment in "human capital" affected youths' employment, earnings, welfare receipt, and other outcomes. In Chapter 6, the analysis compares the experiences of youths in the 13 sites in the demonstration and – to the extent possible given the research design of the study – explores possible explanations for differences among sites.

---

[62](...continued)
include MDRC's study of the impacts of job search alone versus job search plus community work experience in San Diego (Goldman et al., 1986) and Mathematica Policy Research's study of alternative reforms of the unemployment insurance system.

CHAPTER 2

STUDY DESIGN AND SAMPLE CHARACTERISTICS

This chapter presents the study design used to address the research questions posed in Chapter 1, and the characteristics of the research sample used in the analysis. Part I describes the research approach, with special emphasis on the random assignment research approach for assessing the difference the program makes in the lives of young people. Part II describes the research sample for this report.

I. An Overview of the Study Design

Although education and training services for young school dropouts are limited, some youths who entered JOBSTART would have gotten GEDs or high school diplomas, found jobs, increased their earnings, or gotten off welfare on their own even if they had not been in the program. As noted in Chapter 1, to isolate the impact of JOBSTART from other factors that may produce such outcomes, MDRC randomly assigned applicants to experimental and control groups. The two groups were similar except that only the experimental group could receive JOBSTART services. Comparison of the two groups' experiences during the two years after random assignment (the follow-up information available for this report) provided a reliable estimate of the difference the program made during an early post-program period.[1]

---

[1] Sources of the data for the evaluation are discussed in detail in Appendix A. They include enrollment forms completed just prior to random assignment; a management information system (MIS) that provided data on participation in the program; results from the Test of Adult Basic Education (TABE) administered to members of the experimental group; follow-up surveys (for this report) conducted 12 and 24 months after random assignment and (for the final report) approximately 48 months after random assignment; program cost data from a variety of sources; and qualitative data based on interviews with the program staff, field observations of program operations, and focus group discussions with participants.

A. How Random Assignment Was Carried Out

Figure 2.1 shows the steps in the intake and random assignment process.[2] Youths who expressed an interest in program services entered the program through a process that took from one day to one month (10 days on average), depending on the site.[3] Most of the steps were part of the usual JTPA Title IIA (or, in Atlanta, Los Angeles, and Phoenix, the Job Corps) intake procedure; in most sites, only the reading test and random assignment were added for the JOBSTART Demonstration.

The order of steps varied from site to site, as did the division of responsibility between the program operator and the local service delivery area (SDA). The process included:

- **Client recruitment.** JOBSTART was voluntary, so the program operator and, in some cases, the SDA actively recruited youths to apply, using a variety of techniques to meet their enrollment goals. Program staff approached potential recruits through media announcements; mailings to dropouts and welfare recipients; and outreach visits to schools, parks, and other youth gathering places. They distributed posters and fliers advertising program services and sought referrals of eligible youths from JTPA, community organizations, schools, and social service agencies. Recruitment activities frequently took staff beyond the boundaries of the office and the nine-to-five workday. Recruitment through public school referrals or outreach was productive in school-based JOBSTART programs.

- **Informational interview.** In a brief interview, JOBSTART staff explained to potential applicants the program's services and obligations and, often, the random assignment procedures. Some sites also regularly included a tour of their facilities to help recruits understand program services, opportunities, and demands.

- **Assessment.** Program staff assessed whether applicants met the age (17 to 21), educational status (school dropout), and income requirements for JOBSTART. They also ascertained the youths' support service needs and appropriateness for the program, screening out those with problems the program was not equipped to handle. The assessment process was relatively extensive at the Job Corps sites, which had the broadest array of support services. Job Corps staff assessed recruits for emotional problems, drug and alcohol abuse, trouble with the law, unstable living situations, health problems, and motivation. Other sites screened mostly to identify

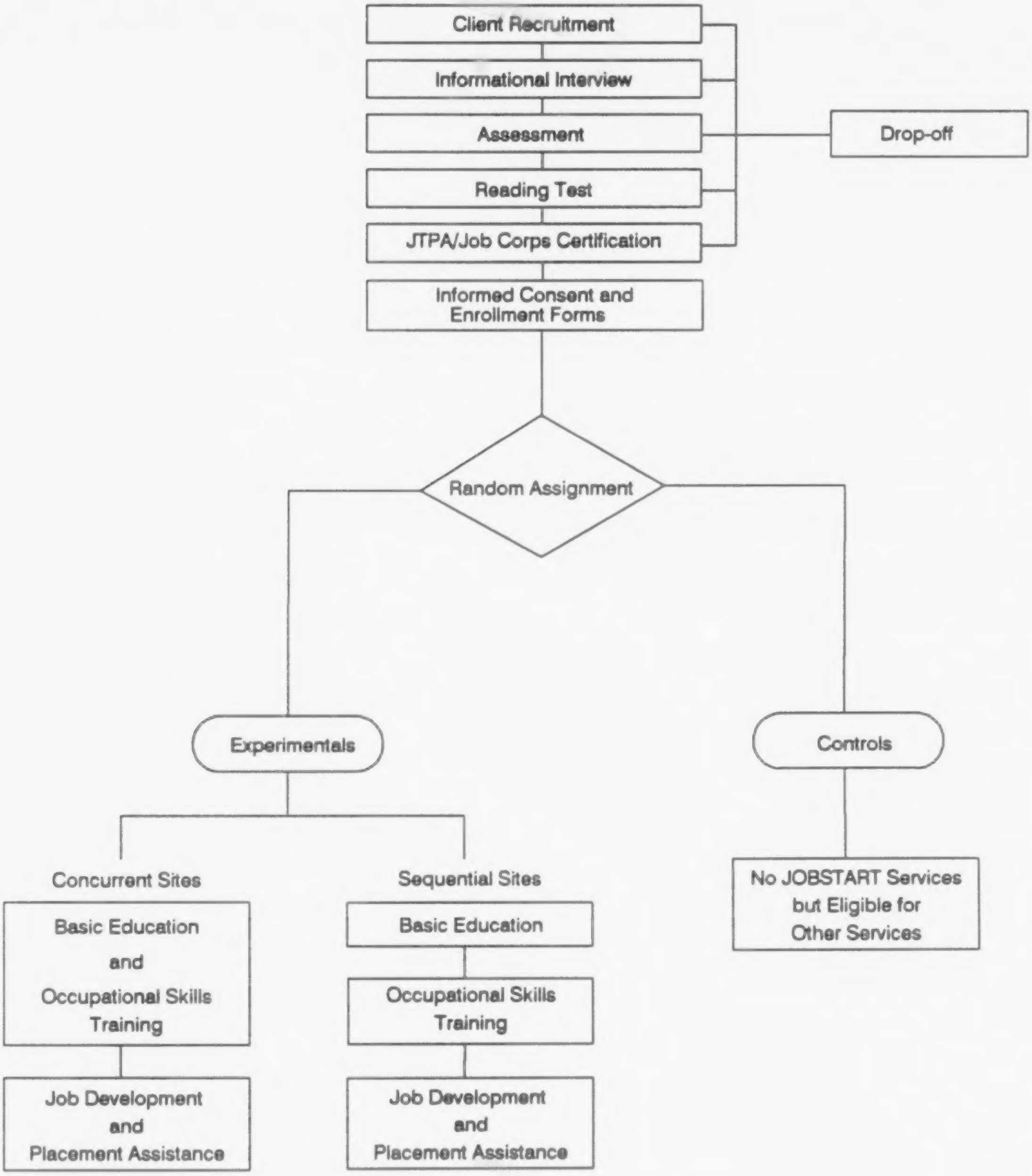
---

[2] See Auspos et al., 1989, for the details.

[3] Sites varied greatly in the amount of initial assessment they conducted before allowing entry into the program. There was also wide variation in state and local interpretation of the documentation needed to establish eligibility for JTPA Title IIA programs.

FIGURE 2.1

THE JOBSTART EVALUATION DESIGN AND SAMPLE FLOW


Client Recruitment
Informational Interview
Assessment
Reading Test
JTPA/Job Corps Certification
Drop-off
Informed Consent and Enrollment Forms
Random Assignment
Experimentals
Controls
Concurrent Sites
Basic Education and Occupational Skills Training
Job Development and Placement Assistance
Sequential Sites
Basic Education
Occupational Skills Training
Job Development and Placement Assistance
No JOBSTART Services but Eligible for Other Services

SOURCE: Adapted from Auspos et al., 1989.

NOTE: Support services such as child care and transportation were offered at both concurrent and sequential sites.

youths who were likely to prove dangerous or disruptive, such as those with evident drug or alcohol problems.

- **Reading test.** Most program operators tested the reading level of recruits early in the intake process to determine that applicants read below the eighth-grade level, as required by JOBSTART eligibility criteria. Four sites (the three Job Corps Centers and CET/San Jose) delayed testing until later in the program, limiting their testing to participants. As noted earlier, sites were permitted to enroll up to 20 percent of their recruits with higher reading scores to help meet enrollment goals. Some sites also set a lower limit or floor – a fourth-, fifth-, or sixth-grade reading level. These program operators felt that the youths would need to read at least at these levels in order to benefit from the education and training services that were available locally.

- **JTPA Title IIA/Job Corps certification.** Recruits had to prove that they fulfilled eligibility criteria for JTPA Title IIA-funded services. At the Job Corps sites, recruits also had to meet Job Corps eligibility criteria. At all the sites, certification of eligibility required proof of residency, age, and economic disadvantage. SDAs at most sites required applicants to provide supporting documentation of all aspects of JTPA Title IIA eligibility for approval of enrollment into JOBSTART. Local regulations and practices affecting the certification process strongly influenced the speed and ease of certification. JTPA Title IIA certification procedures were cited by program operators in six sites as a major bottleneck in the intake and enrollment process.

- **Informed consent form, enrollment form, and random assignment.** After staff described the random assignment process, the applicant signed an informed consent form, agreeing to accept the results of random assignment and to cooperate in follow-up survey interviews. Program or SDA staff then filled out the enrollment form, using information provided by the applicant. Staff then telephoned MDRC, where random assignment was made. Youths entering the experimental group were told to report to classes or, in some sites, to an orientation session. Program staff contacted experimentals who did not appear for program activities, encouraging them to participate and assisting them with needed support services. Applicants assigned to the control group were reminded that they were part of the research project and would be contacted later. They were also told that they could seek services elsewhere on their own.

A total of 2,312 people were randomly assigned: 1,163 to the experimental group and 1,149 to the control group.[4] Sites conducted random assignment over varying periods of time. Connelley Skill Learning Center enrolled the first sample members in August 1985, and the Los Angeles Job Corps enrolled the last sample members in November 1987. Open-entry/open-exit sites continuously recruited applicants to maintain enrollment levels, while sites operating fixed-cycle programs – such as Connelley in Pittsburgh, Chicago Commons, and SER/Corpus Christi – intensified recruitment efforts before the start of classes.

Overall, sites reported that about 89 percent of the youths in the experimental group participated to some extent in JOBSTART. The percentage participating did vary among the sites, from a high of 100 percent at Allentown in Buffalo and El Centro in Dallas to a low of 64 percent at CET/San Jose. (Site-specific participation information is presented in Chapter 3.) Four factors influenced the percentage of experimentals reported to be active in the program:

- **Length of the intake process.** The process of selection into the JOBSTART Demonstration took a relatively short time in many sites, often less than a week. However, at a few sites, the extended checks of eligibility (most important in the Job Corps sites) meant that intake lasted much longer, and in the process some youths who were eventually assigned to the experimental group found other program options or lost interest.

- **Open-entry/open-exit versus fixed-cycle scheduling.** Open-entry programs allow young people to enter and finish at any time, while other programs operate on fixed schedules of class cycles.[5] Youths assigned to the experimental group in fixed-cycle sites might face delays in program start-up, resulting in lower participation rates.

- **Start-up or scheduling problems.** Some sites had unexpected problems getting youths into services. The most notable example was the experience of the early entrants at CET/San Jose, where program slots were not available for up to a month after random assignment because of funding cuts. This delay contributed to this site's experimentals having the lowest rate of participation in JOBSTART services.

---

[4] All but one of the 2,312 youths who were randomly assigned filled out enrollment forms providing pre-program baseline data on age, sex, prior employment, extent of schooling, and other characteristics used in impact calculations and to define key groups within the full sample.

[5] Sites operating open-entry/open-exit programs included Allentown in Buffalo, the Atlanta Job Corps, CET/San Jose, the East Los Angeles Skills Center, El Centro in Dallas, the Los Angeles Job Corps, and the Phoenix Job Corps. EGOS in Denver offered classes on a semester schedule but allowed entry whenever classes were in session.

- **Differences in sites' attendance reporting.** The program elements counted in participation in all sites included education, training, and other activities such as life skills training, work experience, and – in the Job Corps sites – a lengthy orientation. Participation in an extended assessment of training interests at CET/San Jose was not included in reported hours. Therefore, if youths attended this assessment *and nothing else,* their reported hours were zero and they were counted as nonparticipants. This could have affected CET/San Jose's participation rate and reported hours in activities.

B. The Research Samples Used in This Report

Follow-up surveys at 12 and 24 months after random assignment gathered data on outcomes such as participation in education and training programs, educational attainment, employment, earnings, and use of public benefit programs.[6] Of the 2,311 youths in the full research sample, 1,839, or 80 percent, provided 24 months of survey follow-up data and constitute the "impact sample" analyzed in this report in Chapters 4 through 6.[7] The 949 experimentals in this sample are used to examine the implementation of JOBSTART, in Chapter 3, where issues such as participation rates in JOBSTART and its components, and hours and duration of participation, are examined.

C. Key Methodological Issues for the Impact Analysis

For this study to produce unbiased estimates of program impacts, several conditions must be met. These are addressed in the following questions:[8]

1. **Did random assignment lead to a group of experimentals with the same measured pre-program characteristics as the controls?** Random assignment – properly implemented – creates a group of JOBSTART controls with the same pre-program characteristics as JOBSTART experimentals, so that observed differences between

---

[6] As noted in Appendix A, information on participation in JOBSTART was provided by the sites as part of a special management information system (MIS) created for the demonstration. The follow-up surveys collected information on participation in all other education, training, and employment programs for both experimentals and controls.

[7] Most responded to both the 12- and 24-month follow-up surveys (1,604, or 87 percent of responders), while the remainder (235 people) responded to a special combination survey covering the entire 24 months, which was fielded for youths who did not respond to the 12-month survey but were located at 24 months.

[8] For a fuller discussion, see Appendix B.

experimentals and controls in post-random assignment behavior provide unbiased, accurate estimates of program impacts.[9] The information presented in Appendix B (Table B.1) for the 2,311 people randomly assigned shows that there were virtually no measured differences in characteristics between experimentals and controls.[10]

2. **Do experimentals and controls in the impact sample for this report (that is, those with 24 months of survey follow-up) have the same measured pre-program characteristics?** Appendix Table B.2 shows that the 1,839 experimentals and controls in the impact sample are virtually identical in average measured characteristics.

3. **Are those 1,839 sample members with 24 months of survey data representative of the entire JOBSTART sample of 2,311?** Twenty-four months of survey data are available for nearly 80 percent of all the youths who were randomly assigned. Appendix Table B.3 shows that there are some statistically significant differences between those who responded to the surveys and nonresponders. Responders were more likely to be experimentals: 82 percent of experimentals provided 24 months of survey follow-up information compared to 78 percent of controls. Responders were also more likely to have entered the sample at the Allentown in Buffalo site, to have had no criminal convictions between age 16 and baseline, and to have lived with both parents at age 14.

When nonresponse is randomly distributed among members of both the experimental and control groups, it is troublesome only because it reduces the sample size and thus the statistical power to find impacts of a given size.[11] However, when nonresponse is greater among one research group (such as controls) or among members of either research group who have certain characteristics (such as those who, at age 14, lived with their parents), impacts may be biased slightly unless they are corrected for nonresponse.

---

[9] This condition is known as the "internal validity" of the estimate.

[10] The only difference that was statistically significant at the 5 percent level was that experimentals in the sample were slightly more likely than controls to be a part of an AFDC case headed by another member of their household. On a site-by-site basis, the 24-month JOBSTART impact sample consists of 26 separate groups of experimentals and controls. If experimentals in any site are compared with controls in that site, the internal validity of site impacts may be assessed. As would be expected in 13 relatively small subsamples of the full 24-month impact sample of 1,839, there are a few experimental-control differences in demographic characteristics within individual sites.

[11] Randomly distributed nonresponse does not alter the expected values of adjusted mean outcomes, and thus does not bias impacts.

The impacts presented in this report do not include any corrections for the differences in survey responders and nonresponders just described.[12] The success of attempts to implement such corrections is uncertain, and the differential response rates found do not seem large enough to warrant such measures, which could introduce biases of their own. The high overall response rate of 80 percent makes findings from the 24-month impact sample representative of a very broad group of the full sample.

4. **Did most experimentals receive JOBSTART services, and did relatively few controls receive them or any equivalent services?** For this condition to be met, experimentals must participate in JOBSTART, and controls must be excluded from JOBSTART and not find equivalent services elsewhere in their community. As discussed above, nearly 90 percent of experimentals were active in JOBSTART. For controls, random assignment procedures were followed, and virtually no controls were served in JOBSTART programs. In addition, Chapter 4's analysis of the receipt of education and training services from all sources indicates that controls did not find an equivalent level of services elsewhere. For example, in the first year after random assignment, 91 percent of experimentals and 29 percent of controls participated in some type of education or training activity. As this and the other measures used in Chapter 4 indicate, experimentals did receive a noticeably greater total amount of employment and training services. But it is important to keep in mind that controls were not an *unserved* group; many received substantial services from sources other than the JOBSTART programs. The impact findings presented in Chapters 4 through 6 of this report, therefore, should be interpreted as measuring the incremental impact of the services received by experimentals above the level of services received by controls.

5. **Do the impacts per person assigned to the experimental group differ greatly from the impacts per person participating in JOBSTART?** Some of those who were randomly assigned to the experimental group (the group given access to the JOBSTART program) never participated. However, they were still included as part of the experimental

---

[12] The most flexible correction for nonresponse is incorporation of an additional equation for survey response into a two-equation system with the impact equation.

group when average impacts were calculated, somewhat "watering down" the impacts.[13] Fortunately, the percentage of nonparticipants was small (only 11 percent of the 949 experimentals in the impact sample), so including them "diluted" the impacts only slightly. In other words, while the impacts refer to all surveyed experimentals (nonparticipants as well as participants), they would be only slightly higher if they were adjusted to apply to surveyed participants only.[14]

II. The Characteristics of the JOBSTART Youths

Examining the pre-program experiences and characteristics of the young people in the JOBSTART sample is important for three reasons. First, it shows whether the sites in the demonstration succeeded in enrolling economically disadvantaged young people with poor skills who were the target group for the demonstration. Second, it permits a comparison of the JOBSTART youths with those served by other important employment and training programs. Third, much of the analysis in this report moves beyond results for the full sample of JOBSTART youths to examine whether and how the program worked differently for subgroups of young people (especially young males, mothers, and other women), and understanding the pre-program characteristics of these groups is the first step in this analysis.

The third point is important because groups defined by a single characteristic (such as gender, age, prior employment, or the type of site to which they applied) may vary on other characteristics as well. Young women in the sample, for example, may have had less prior employment experience and more prior public assistance receipt than did the men in the sample. Sites offering education followed by training at another agency may attract very different applicants than those known for their training courses. Understanding the *combination* of characteristics associated with subgroups such as men or women or types of sites helps prevent misinterpretations of any observed differences in program participation and

---

[13] If the nonparticipants had not been counted, the experimental group would no longer have been truly comparable to the control group. Including them in the impact calculations was designed to avoid a form of "selection bias" -- in this case, caused by those who had "selected themselves" out of their chance to join the JOBSTART program or were discouraged by program staff.

[14] See Appendix B for details on such adjustments. In some sites, nonparticipation rates were considerably higher than the 11 percent for the entire impact sample, so the difference between impacts per experimental and per participant is greater.

59

effectiveness. With this goal of the report in mind, Part II of this chapter summarizes the characteristics of the youths at each site and of key subgroups of young people that will be examined throughout the remaining chapters.[15]

A. The Characteristics of the Impact Sample of JOBSTART Youths

Table 2.1 provides detailed background information on the impact sample of JOBSTART youths and shows that the sites in the demonstration succeeded in recruiting the intended target group. The column labeled "all sites" shows the characteristics of the entire impact sample; the remaining columns are discussed in Part IIB of this chapter. The sample is made up of slightly more women than men; most of the sample are members of minority groups and are unmarried; nearly three-fourths are under 20 years of age; slightly fewer than half did not work during the year prior to random assignment; and about 60 percent left school before the eleventh grade.[16]

JOBSTART participants appear to have been more disadvantaged than the majority of youths served nationwide by JTPA Title IIA programs during the period JOBSTART was in operation. In the effort to serve those youths at risk of chronic unemployment, JOBSTART worked exclusively with dropouts, a segment of the youth population that makes up a relatively small part of JTPA Title IIA enrollees. Even when the comparison of participants is limited to young dropouts, it appears that JOBSTART reached a more disadvantaged population than did most JTPA Title IIA-funded programs.[17]

---

[15] More detailed comparisons are included in Appendix C.

[16] The only real divergence from the intended target group occurred because a slightly higher than planned percentage of the youths read at the eighth-grade level or above. This happened because of the educational testing practices of some sites and is not shown in Table 2.1 because of inconsistent data among the sites. Some sites did not administer the reading test as part of the initial assessment of suitability for JOBSTART, which occurred before random assignment.

[17] Approximately 56 percent of JOBSTART participants were receiving some form of public assistance at the time they entered the program, compared to 39 percent of young dropouts served by JTPA Title IIA programs. Moreover, the proportion of JOBSTART participants who received AFDC (38 percent) was much higher than that of young dropouts in other JTPA Title IIA programs (21 percent). This higher rate of welfare receipt partly reflects the fact that a greater proportion of JOBSTART participants were young women (53 percent) compared to the dropout group participating in other JTPA Title IIA programs (45 percent females). Also, minorities were much more heavily represented in JOBSTART than in JTPA Title IIA-funded services for young dropouts nationally. Hispanic dropouts constituted 44 percent of JOBSTART participants but only 14 percent of JTPA Title IIA dropouts, and JOBSTART served proportionally more black dropouts (46 percent) than did other JTPA Title IIA programs (34 percent).

TABLE 2.1

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY SITE

Characteristic and Subgroups	All 13 Sites	Concurrent							
		Atlanta Job Corps	CET/ San Jose	Chicago Commons	Connelley (Pittsburgh)	East LA Skills Center	EGOS (Denver)	Phoenix Job Corps	SER/ Corpus Christi
Gender									
Women	52.6%***	59.0%	49.3%	43.2%	53.3%	44.0%	64.5%	51.5%	39.4%
Men	47.4	41.0	50.7	56.8	46.7	56.0	35.5	48.5	60.6
Ethnicity									
White, non-Hispanic	8.4***	3.3	12.5	4.1	8.2	1.0	11.5	23.1	8.1
Black, non-Hispanic	45.7	96.7	4.6	81.1	91.8	0.0	30.6	17.7	4.7
Hispanic	42.6	0.0	73.7	14.9	0.0	95.0	55.2	53.8	87.3
Other	3.3	0.0	9.2	0.0	0.0	4.0	2.7	5.4	0.0
Ethnicity, by gender									
Women									
White, non-Hispanic	4.6***	3.3	7.2	2.7	3.3	1.0	5.5	12.3	3.8
Black, non-Hispanic	24.2	55.7	2.0	32.4	50.0	0.0	23.5	4.6	3.0
Hispanic	22.5	0.0	38.8	8.1	0.0	41.0	33.3	32.3	32.6
Other	1.4	0.0	1.3	0.0	0.0	2.0	2.2	2.3	0.0
Men									
White, non-Hispanic	3.9	0.0	5.3	1.4	4.9	0.0	6.0	10.8	4.2
Black, non-Hispanic	21.5	41.0	2.6	48.6	41.8	0.0	7.1	13.1	1.7
Hispanic	20.1	0.0	34.9	6.8	0.0	54.0	21.9	21.5	54.7
Other	1.9	0.0	7.9	0.0	0.0	2.0	0.5	3.1	0.0
Parental status									
Women living with own child(ren)									
No	26.3***	29.5	42.8	18.9	19.0	32.0	27.9	23.1	16.9
Yes	26.3	29.5	6.6	24.3	34.2	12.0	36.6	28.5	22.5
Men who have own child(ren)									
No	41.6	39.3	45.4	39.2	37.0	52.0	33.3	43.1	51.7
Yes	5.8	1.6	5.3	17.6	9.8	4.0	2.2	5.4	8.9
Employed within past year									
No	47.3***	37.7	36.8	54.1	28.8	55.0	39.9	55.4	33.5
Yes	52.7	62.3	63.2	45.9	71.2	45.0	60.1	44.6	66.5
Prior employment, by gender									
Women employed within past year									
No	29.7***	29.5	22.4	27.0	18.5	28.0	31.7	36.2	21.6
Yes	22.9	29.5	27.0	16.2	34.8	16.0	32.8	15.4	17.8
Men employed within past year									
No	17.6	8.2	14.5	27.0	10.3	27.0	8.2	19.2	11.9
Yes	29.8	32.8	36.2	29.7	36.4	29.0	27.3	29.2	48.7
Left school in grade 11 or 12									
No	58.6***	59.0	38.8	43.2	64.1	69.0	55.2	60.8	75.0
Yes	41.4	41.0	61.2	56.8	35.9	31.0	44.8	39.2	25.0
Sample size	1,839	61	152	74	184	100	183	130	236

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroups	All 13 Sites	Sequential/In-House		Sequential/Brokered		
		El Centro (Dallas)	LA Job Corps	Allen-town (Buffalo)	BSA (NYC)	CREC (Hartford)
Gender						
Women	52.6%***	54.2%	59.6%	58.6%	44.5%	64.4%
Men	47.4	45.8	40.4	41.4	55.5	35.6
Ethnicity						
White, non-Hispanic	8.4***	6.5	3.2	14.3	3.4	4.6
Black, non-Hispanic	45.7	70.3	50.0	77.1	67.2	56.3
Hispanic	42.6	21.9	34.4	7.9	28.6	39.1
Other	3.3	1.3	12.4	0.7	0.8	0.0
Ethnicity, by gender						
Women						
White, non-Hispanic	4.6***	4.5	1.4	9.3	1.7	2.3
Black, non-Hispanic	24.2	34.2	28.9	43.6	26.1	32.2
Hispanic	22.5	15.5	22.9	5.0	16.8	29.9
Other	1.4	0.0	6.4	0.7	0.0	0.0
Men						
White, non-Hispanic	3.9	1.9	1.8	5.0	1.7	2.3
Black, non-Hispanic	21.5	36.1	21.1	33.6	41.2	24.1
Hispanic	20.1	6.5	11.5	2.9	11.8	9.2
Other	1.9	1.3	6.0	0.0	0.8	0.0
Parental status						
Women living with own child(ren)						
No	26.3***	21.3	29.4	25.7	28.6	36.8
Yes	26.3	32.9	30.3	32.9	16.0	27.6
Men who have own child(ren)						
No	41.6	41.9	39.4	30.0	53.8	31.0
Yes	5.8	3.9	0.9	11.4	1.7	4.6
Employed within past year						
No	47.3***	45.2	75.2	57.9	64.7	31.0
Yes	52.7	54.8	24.8	42.1	35.3	69.0
Prior employment, by gender						
Women employed within past year						
No	29.7***	27.7	47.2	39.3	29.4	24.1
Yes	22.9	26.5	12.4	19.3	15.1	40.2
Men employed within past year						
No	17.6	17.4	28.0	18.6	35.3	6.9
Yes	29.8	28.4	12.4	22.9	20.2	28.7
Left school in grade 11 or 12						
No	58.6***	67.7	41.3	53.6	60.5	74.7
Yes	41.4	32.3	58.7	46.4	39.5	25.3
Sample size	1,839	155	218	140	119	87

(continued)

TABLE 2.1 (continued)

		Concurrent							
Characteristic and Subgroups	All 13 Sites	Atlanta Job Corps	CET/ San Jose	Chicago Commons	Connelley (Pittsburgh)	East LA Skills Center	EGOS (Denver)	Phoenix Job Corps	SER/ Corpus Christi
Received occupational training within past year									
No	83.1%***	65.6%	90.1%	90.5%	66.8%	86.0%	91.8%	94.6%	69.5%
Yes	16.9	34.4	9.9	9.5	33.2	14.0	8.2	5.4	30.5
Age									
16-19	73.9***	77.0	78.9	47.3	54.9	78.0	76.5	86.2	70.3
20 or 21	26.1	23.0	21.1	52.7	45.1	22.0	23.5	13.8	29.7
Marital status									
Ever married	9.5***	6.6	12.5	1.4	3.8	4.0	7.1	10.8	29.7
Never married	90.5	93.4	87.5	98.6	96.2	96.0	92.9	89.2	70.3
Living in own household or with boy/girlfriend									
No	81.6***	88.5	87.5	78.4	75.5	90.0	77.0	85.4	76.3
Yes	18.4	11.5	12.5	21.6	24.5	10.0	23.0	14.6	23.7
Own AFDC case or receiving General Assistance									
No	73.1***	67.2	90.8	47.3	56.5	76.0	73.2	83.1	86.9
Yes	26.9	32.8	9.2	52.7	43.5	24.0	26.8	16.9	13.1
Own AFDC case									
No	78.6***	78.7	94.7	73.0	68.5	79.0	74.9	83.8	88.6
Yes	21.4	21.3	5.3	27.0	31.5	21.0	25.1	16.2	11.4
Receiving Food Stamps									
No	62.2***	63.9	90.1	39.2	25.5	68.0	58.5	76.2	68.6
Yes	37.8	36.1	9.9	60.8	74.5	32.0	41.5	23.8	31.4
Arrested since age 16									
No	85.2***	88.5	76.3	81.1	89.7	84.0	83.1	87.7	78.8
Yes	14.8	11.5	23.7	18.9	10.3	16.0	16.9	12.3	21.2
Lived with both parents at age 14									
No	65.1***	75.4	52.0	74.3	79.9	54.0	59.6	46.2	45.3
Yes	34.9	24.6	48.0	25.7	20.1	46.0	40.4	53.8	54.7
Sample size	1,839	61	152	74	184	100	183	130	236

(continued)

TABLE 2.1 (continued)

Characteristic and Subgroups	All 13 Sites	Sequential/In-House El Centro (Dallas)	Sequential/In-House LA Job Corps	Sequential/Brokered Allen-town (Buffalo)	Sequential/Brokered BSA (NYC)	Sequential/Brokered CREC (Hartford)
Received occupational training within past year						
No	83.1%***	89.0%	91.7%	82.9%	79.8%	82.8%
Yes	16.9	11.0	8.3	17.1	20.2	17.2
Age						
16-19	73.9***	83.9	77.5	73.6	73.9	80.5
20 or 21	26.1	16.1	22.5	26.4	26.1	19.5
Marital status						
Ever married	9.5***	11.6	5.0	4.3	1.7	5.7
Never married	90.5	88.4	95.0	95.7	98.3	94.3
Living in own household or with boy/girlfriend						
No	81.6***	91.6	86.7	59.3	95.0	77.0
Yes	18.4	8.4	13.3	40.7	5.0	23.0
Own AFDC case or receiving General Assistance						
No	73.1***	84.5	67.0	52.1	77.3	70.1
Yes	26.9	15.5	33.0	47.9	22.7	29.9
Own AFDC case						
No	78.6***	84.5	69.3	67.9	83.2	73.6
Yes	21.4	15.5	30.7	32.1	16.8	26.4
Receiving Food Stamps						
No	62.2***	85.8	69.7	30.0	62.2	62.1
Yes	37.8	14.2	30.3	70.0	37.8	37.9
Arrested since age 16						
No	85.2***	90.3	90.4	87.1	89.9	80.5
Yes	14.8	9.7	9.6	12.9	10.1	19.5
Lived with both parents at age 14						
No	65.1***	71.6	75.2	80.7	70.6	79.3
Yes	34.9	28.4	24.8	19.3	29.4	20.7
Sample size	1,839	155	218	140	119	87

(continued)

TABLE 2.1 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Nationwide, 80 percent of Job Corpsmembers were school dropouts in program year 1986, when the JOBSTART Demonstration was in operation, but their other characteristics suggest greater barriers to employment than the JOBSTART youths faced. Job Corpsmembers tended to be younger than JOBSTART participants: 42 percent were age 16 or under in 1986 compared to 25 percent in JOBSTART. Sixty-one percent read at the sixth-grade level or below at entry into the Job Corps compared to 52 percent in JOBSTART.[18] On the other hand, a higher proportion of JOBSTART participants were receiving public assistance and were members of minority groups than were Job Corpsmembers. The residential character of the Job Corps program also introduces another difference: All residential Corpsmembers are willing and able to live away from home, but an unknown – though probably large – portion of JOBSTART members would not fall into this category.[19]

These comparisons suggest that JOBSTART sites did succeed in attracting disadvantaged, young school dropouts, as intended in the demonstration. However, these young people were not among the most disadvantaged youths: Relatively few reported at program intake that they had criminal arrests; most were not teenage parents; and about half had worked during the year before random assignment.[20] In summary, the JOBSTART youths probably fell between the typical JTPA and Job Corps participant in initial skills levels and job readiness.

B. Site Differences in Sample Characteristics

When individuals with certain characteristics are largely concentrated in one or a few sites, the influences of their individual characteristics on program implementation and impacts are "confounded" with the influence of site characteristics. This is virtually a non-issue with regard to the proportion of experimentals and controls at the sites: All sites had approximately equal proportions of the two groups. Not so with ethnicity, in particular: The proportion of black sample members ranged from zero at the East Los Angeles Skills Center to 97 percent at the Atlanta Job Corps, as shown in the individual site columns of Table 2.1. Thus, while the proportion of experimentals and controls is almost independent of site, the influence of ethnicity is much more confounded with the influence of site characteristics.[21] Because of this, simple experimental-control comparisons of post-program outcomes for the full sample can confidently be interpreted as resulting from experimentals' access to JOBSTART, but there is not the same confidence about comparisons of experimental-control experiences for ethnic subgroups (which could be heavily influenced by different program structures, labor market conditions, or other important background factors completely external to JOBSTART). Most characteristics lie somewhere between independence and confounding. Thus, unless special techniques are used to remove associations between site and other characteristics, impact comparisons for many subgroups and site groupings may be misleading.

---

[18] In JOBSTART, JTPA performance standards and practices led some JTPA Title IIA-funded sites to exclude youths with very low reading scores; the Job Corps sites in JOBSTART appeared to include a higher proportion of youths with very low reading scores than did other sites.

[19] The difficulties encountered in implementing an unsuccessful random assignment study of the residential versus nonresidential Job Corps illustrate the importance of this difference. The study originally assumed that a substantial portion of the Job Corps applicant pool would be indifferent as to whether they got into a residential or nonresidential program, and the study proposed to randomly assign members of this group to the two program types. This "indifferent" group turned out to be too small a portion of applicants for the study to proceed.

[20] It is very likely that youths underreported past arrests, since they were asked about this at program intake and may have assumed that a positive response would lower their chances of getting into the program. Also, they may not have wanted to provide this information to staff, whom they did not yet know. In addition, only 106 men (or 12 percent of men) report being a father.

Fortunately, in view of the heavy emphasis this report places on comparisons of outcomes by gender, there is much less cause for concern in making comparisons of impacts for gender-defined subgroups than for those based on ethnicity. The sample was 47 percent male overall, and men were distributed across sites much more evenly than were blacks or Hispanics.[22] A more refined analysis of gender, appearing throughout this report, splits the sample further, based on parenting status. This creates categories that can signal differences in barriers to employment or willingness to sacrifice in order to obtain a steady source of earned income. Overall, half of the women lived with children of their own, and half did not; among men, about one-seventh reported that they already were parents at baseline, and six-sevenths reported having no children at that time. The proportions of parents among women and men in the sample did vary among the sites, but the variation was much less than was the case for ethnic groups.[23]

---

[21] Overall, most sample members were black (46 percent) or Hispanic (43 percent). In six sites, more than two-thirds of the participants were black, while in three, more than two-thirds were Hispanic. The 8 percent overall proportion of white non-Hispanic sample members was disproportionately concentrated at the Phoenix Job Corps and, to a lesser extent, at Allentown in Buffalo, CET/San Jose, and EGOS in Denver.

[22] The proportion of males in each site ranged from a high of 61 percent at SER/Corpus Christi to a low of 36 percent at EGOS in Denver. In addition to SER/Corpus Christi, four other sites had male majorities: Chicago Commons, the East Los Angeles Skills Center, BSA in New York City, and CET/San Jose.

Site differences were also large for several other subgroups of the research sample:

- **The amount of prior schooling varied among the sites more than gender and parenting status.** Large differences in baseline educational attainment are important to bear in mind when examining rates of post-program GED attainment. Other factors aside, those who were closer to finishing high school at baseline are more likely to have received a GED at follow-up.

- **Employment during the year before random assignment varied among the sites even more than prior schooling.** Holding all other observed factors constant, not having worked recently may signal either greater barriers to employment or more interest in schooling than in employment.[24]

- **Public assistance receipt varied greatly.** The percentage of a site's sample receiving public assistance may be a good indication of the relative income and job-readiness of the young people there.[25]

In subsequent chapters, program impacts for subgroups of the impact sample are presented. These subgroups are defined based on pre-random assignment (that is, pre-program) characteristics, and two types of analyses are presented.[26] One approach splits the entire sample into groups defined by a characteristic such as gender. This "split file" subgroup analysis does not control for other measured differences among the groups, such as their site location or prior work experience. If a subgroup is concentrated in a few sites, as is the case for Hispanics, then the "split file" results may be reflecting site differences as much as subgroup differences. Since men and women are not concentrated in particular sites, the split file analysis presented for these groups in later chapters is appropriate. The second type of subgroup analysis presents results for designated subgroups that are statistically adjusted to account for other measured pre-program differences in the groups besides the characteristic used in defining them. For example, it presents results for ethnic subgroups controlling for differences in measured characteristics other than ethnicity. This analysis does control for site differences and thus can be used for subgroups that are relatively concentrated among a few sites.

---

[23] The proportion of men acknowledging fatherhood ranged from almost none at the Los Angeles Job Corps, BSA in New York City, and the Atlanta Job Corps, to 18 percent at Chicago Commons. At CET/San Jose, the East Los Angeles Skills Center, and BSA in New York City, JOBSTART women were more likely not to be custodial mothers than to be living with their own children, while at Connelley in Pittsburgh, El Centro in Dallas, SER/Corpus Christi, Chicago Commons, EGOS in Denver, the Phoenix Job Corps, and Allentown in Buffalo, the opposite was true.

[24] More men than women worked in the year prior to random assignment, but for each group the proportion working also varied greatly among the sites. In most sites, the majority of women had not worked in the year prior to random assignment, with the ratio of nonworking women to working women going above two-to-one at the Phoenix Job Corps. However, Connelley in Pittsburgh and CREC in Hartford were notable exceptions, with substantial majorities of women having had prior-year work experience in those sites. Men's prior employment profiles by site were quite the opposite, with ratios of employed to nonemployed as high as four-to-one at SER/Corpus Christi, CREC in Hartford, and the Atlanta Job Corps. Only among Los Angeles Job Corps men and men at BSA in New York City did nonworkers outnumber workers, with the former's ratio of nonworkers to workers exceeding two-to-one.

[25] The average initial reading level on entering the program also varied among the sites. This is not discussed in detail because initial test scores are not available in all sites.

[26] Defining subgroups based on pre-random assignment characteristics is necessary in order to maintain the legitimacy of comparisons of experimentals and controls. For example, those who had not worked in the year prior to random assignment were just as likely to be randomly assigned to the experimental group as to the control group, making comparisons of experimentals and controls with this characteristic appropriate.

C. Sample Differences for Key Site Groupings

In view of this report's special emphasis on sites' delivery systems for employment and training – whether concurrent, sequential/in-house, or sequential/brokered – Table 2.2 collapses the 13 site columns of Table 2.1 into three columns, one for each type of delivery system.[27] Averaging data for sites in broad categories destroys much of the observed site variation – particularly regarding ethnicity, receipt of welfare and Food Stamps, parenting status, amount of schooling, and prior-year employment.

Some of the observed site variation remains, however. Sample members at sequential/in-house sites were more likely to be custodial mothers, less likely to have acknowledged fatherhood on the enrollment form, less likely to have worked during the prior year, and more likely to have quit school during junior or senior year than were sample members in other sites. Sample members in concurrent sites were older, more likely to have worked during the prior year, less likely never to have been married, less likely to be receiving

---

[27] The first eight columns of Table 2.1 are collapsed into the first column of Table 2.2, for concurrent sites. The next two columns of Table 2.1 – for El Centro in Dallas and the Los Angeles Job Corps – are collapsed into the "sequential/in-house" column of Table 2.2. Finally, the last three columns of Table 2.1 – for Allentown in Buffalo, BSA in New York City, and CREC in Hartford – are collapsed into the "sequential/brokered" column of Table 2.2.

TABLE 2.2

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY PROGRAM STRUCTURE

Characteristic and Subgroups	Sample Size	Concurrent	Sequential/ In-House	Sequential/ Brokered	All Categories	p[a]
Gender						
Women	968	50.3%	57.4%	55.2%	52.6%**	0.034
Men	871	49.7	42.6	44.8	47.4	
Ethnicity						
White, non-Hispanic	155	9.8	4.6	8.1	8.4***	0.000
Black, non-Hispanic	840	34.4	58.4	68.5	45.7	
Hispanic	783	53.1	29.2	22.8	42.6	
Other	61	2.7	7.8	0.6	3.3	
Ethnicity, by gender						
Women						
White, non-Hispanic	84	5.1	2.7	4.9	4.6***	0.000
Black, non-Hispanic	445	18.7	31.1	34.7	24.2	
Hispanic	413	25.5	19.8	15.3	22.5	
Other	26	1.0	3.8	0.3	1.4	
Men						
White, non-Hispanic	71	4.7	1.9	3.2	3.9	
Black, non-Hispanic	395	15.7	27.3	33.8	21.5	
Hispanic	370	27.6	9.4	7.5	20.1	
Other	35	1.7	4.0	0.3	1.9	
Parental status						
Women living with own child(ren)						
No	484	25.4	26.0	29.5	26.3***	0.005
Yes	484	24.8	31.4	25.7	26.3	
Men who have own child(ren)						
No	765	42.9	40.5	38.4	41.6	
Yes	106	6.8	2.1	6.4	5.8	
Employed within past year						
No	870	40.3	62.7	53.5	47.3***	0.000
Yes	969	59.7	37.3	46.5	52.7	
Prior employment, by gender						
Women employed within past year						
No	547	25.9	39.1	32.1	29.7***	0.000
Yes	421	24.4	18.2	23.1	22.9	
Men employed within past year						
No	323	14.4	23.6	21.4	17.6	
Yes	548	35.4	19.0	23.4	29.8	
Sample size	1,839	1,120	373	346		

(continued)

TABLE 2.2 (continued)

Characteristic and Subgroups	Sample Size	Concurrent	Sequential/ In-House	Sequential/ Brokered	All Categories	p^a
Left school in grade 11 or 12						
No	1,078	59.9%	52.3%	61.3%	58.6%**	0.019
Yes	761	40.1	47.7	38.7	41.4	
Received occupational training within past year						
No	1,529	81.1	90.6	81.8	83.1***	0.000
Yes	310	18.9	9.4	18.2	16.9	
Age						
16-19	1,359	71.3	80.2	75.4	73.9***	0.003
20 or 21	480	28.7	19.8	24.6	26.1	
Marital status						
Ever married	174	11.8	7.8	3.8	9.5***	0.000
Never married	1,665	88.2	92.2	96.2	90.5	
Living in own household or with boy/girlfriend						
No	1,500	80.9	88.7	76.0	81.6***	0.000
Yes	339	19.1	11.3	24.0	18.4	
Own AFDC case or receiving General Assistance						
No	1,344	75.1	74.3	65.3	73.1***	0.001
Yes	495	24.9	25.7	34.7	26.9	
Own AFDC case						
No	1,446	80.9	75.6	74.6	78.6**	0.012
Yes	393	19.1	24.4	25.4	21.4	
Receiving Food Stamps						
No	1,143	61.4	76.4	49.1	62.2***	0.000
Yes	696	38.6	23.6	50.9	37.8	
Arrested since age 16						
No	1,567	83.1	90.3	86.4	85.2***	0.002
Yes	272	16.9	9.7	13.6	14.8	
Lived with both parents at age 14						
No	1,198	58.7	73.7	76.9	65.1***	0.000
Yes	641	41.3	26.3	23.1	34.9	
Sample size	1,839	1,120	373	346		

(continued)

TABLE 2.2 (continued)

Characteristic and Subgroups	Sample Size	Concurrent	Sequential/ In-House	Sequential/ Brokered	All Categories	p[a]
Site						
Concurrent						
Atlanta Job Corps	61	5.4%	0.0%	0.0%	3.3%***	0.000
CET/San Jose	152	13.6	0.0	0.0	8.3	
Chicago Commons	74	6.6	0.0	0.0	4.0	
Connelley (Pittsburgh)	184	16.4	0.0	0.0	10.0	
East LA Skills Center	100	8.9	0.0	0.0	5.4	
EGOS (Denver)	183	16.3	0.0	0.0	10.0	
Phoenix Job Corps	130	11.6	0.0	0.0	7.1	
SER/Corpus Christi	236	21.1	0.0	0.0	12.8	
Sequential/in-house						
El Centro (Dallas)	155	0.0	41.6	0.0	8.4	
LA Job Corps	218	0.0	58.4	0.0	11.9	
Sequential/brokered						
Allentown (Buffalo)	140	0.0	0.0	40.5	7.6	
BSA (NYC)	119	0.0	0.0	34.4	6.5	
CREC (Hartford)	87	0.0	0.0	25.1	4.7	
Sample size	1,839	1,120	373	346		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The column labeled "p" is the statistical significance level of differences among groups in distributions of characteristics: that is, p is the probability that observed proportions in each subgroup differ from one column to another only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

AFDC, more likely to have lived with two parents at age 14, less likely to be black, and more likely to be Hispanic than were sample members in other sites.[28]

D. Gender Differences in Baseline Characteristics

An important question in evaluating JOBSTART is whether participation and program impacts varied by gender. A first step toward understanding these gender differences is to examine the other characteristics of the various gender-defined groups. Table 2.3 shows that men and women in the impact sample were similar in many characteristics, including age, ethnic background, educational attainment, and initial reading levels. However, men were more likely to have had recent work experience and vocational training and to have been arrested since age 16. They were less likely to have been married, to be a parent, and to be receiving public assistance.

Most of the differences between men and women just enumerated are due mainly to differences between mothers and other members of the sample. It is the mothers who are least likely to have worked in the year before random assignment, most likely to have lived on their own, most likely to have received AFDC and Food Stamps, and least likely to have lived with both parents at age 14. Because of these clear differences in initial characteristics between young mothers and other young women, the gender-based subgroup analysis that follows divides the female sample into these two groups. However, in most of the analysis, all men are grouped together because of the small number of men reporting that they were parents at baseline.

---

[28] There may also be unobserved differences. For example, youths attracted to sequential/brokered programs run by community-based educational institutions may be more interested in passing the GED examination than are youths at concurrent sites run by training agencies.

TABLE 2.3

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY GENDER AND PARENTAL STATUS

Characteristic and Subgroups	Sample Size	Women: Living with Own Child(ren)	Women: All Others	Men: Do Not Have Own Child(ren)	Men: Have Own Child(ren)	All Categories	p^{a}
Ethnicity							
White, non-Hispanic	155	7.2%	10.1%	9.0%	1.9%	8.4%***	0.000
Black, non-Hispanic	840	54.1	37.8	43.5	58.5	45.7	
Hispanic	783	37.8	47.5	43.0	38.7	42.6	
Other	61	0.8	4.5	4.4	0.9	3.3	
Ethnicity, by gender							
Women							
White, non-Hispanic	84	7.2	10.1	0.0	0.0	4.6***	0.000
Black, non-Hispanic	445	54.1	37.8	0.0	0.0	24.2	
Hispanic	413	37.8	47.5	0.0	0.0	22.5	
Other	26	0.8	4.5	0.0	0.0	1.4	
Men							
White, non-Hispanic	71	0.0	0.0	9.0	1.9	3.9	
Black, non-Hispanic	395	0.0	0.0	43.5	58.5	21.5	
Hispanic	370	0.0	0.0	43.0	38.7	20.1	
Other	35	0.0	0.0	4.4	0.9	1.9	
Employed within past year							
No	870	62.4	50.6	38.2	29.2	47.3***	0.000
Yes	969	37.6	49.4	61.8	70.8	52.7	
Prior employment, by gender							
Women employed within past year							
No	547	62.4	50.6	0.0	0.0	29.7***	0.000
Yes	421	37.6	49.4	0.0	0.0	22.9	
Men employed within past year							
No	323	0.0	0.0	38.2	29.2	17.6	
Yes	548	0.0	0.0	61.8	70.8	29.8	
Sample size	1,839	484	484	765	106		

(continued)

TABLE 2.3 (continued)

Characteristic and Subgroups	Sample Size	Women: Living with Own Child(ren)	Women: All Others	Men: Do Not Have Own Child(ren)	Men: Have Own Child(ren)	All Categories	p^a
Left school in grade 11 or 12							
No	1,078	61.6%	58.1%	57.3%	57.5%	58.6%	0.487
Yes	761	38.4	41.9	42.7	42.5	41.4	
Received occupational training within past year							
No	1,529	88.0	86.2	79.1	76.4	83.1***	0.000
Yes	310	12.0	13.8	20.9	23.6	16.9	
Age							
16-19	1,359	61.6	85.1	77.9	50.0	73.9***	0.000
20 or 21	480	38.4	14.9	22.1	50.0	26.1	
Marital status							
Ever married	174	19.6	6.0	3.1	24.5	9.5***	0.000
Never married	1,665	80.4	94.0	96.9	75.5	90.5	
Living in own household or with boy/girlfriend							
No	1,500	55.8	86.8	94.9	79.2	81.6***	0.000
Yes	339	44.2	13.2	5.1	20.8	18.4	
Own AFDC case or receiving General Assistance							
No	1,344	37.6	83.9	87.8	79.2	73.1***	0.000
Yes	495	62.4	16.1	12.2	20.8	26.9	
Own AFDC case							
No	1,446	39.9	89.9	94.5	89.6	78.6***	0.000
Yes	393	60.1	10.1	5.5	10.4	21.4	
Sample size	1,839	484	484	765	106		

(continued)

TABLE 2.3 (continued)

		Women		Men			
Characteristic and Subgroups	Sample Size	Living with Own Child(ren)	All Others	Do Not Have Own Child(ren)	Have Own Child(ren)	All Categories	p^a
Receiving Food Stamps							
No	1,143	41.1%	67.6%	72.8%	56.6%	62.2%***	0.000
Yes	696	58.9	32.4	27.2	43.4	37.8	
Arrested since age 16							
No	1,567	96.1	93.8	75.3	67.9	85.2***	0.000
Yes	272	3.9	6.2	24.7	32.1	14.8	
Lived with both parents at age 14							
No	1,198	73.8	64.7	59.1	71.7	65.1***	0.000
Yes	641	26.2	35.3	40.9	28.3	34.9	
Site							
Concurrent							
Atlanta Job Corps	61	3.7	3.7	3.1	0.9	3.3***	0.000
CET/San Jose	152	2.1	13.4	9.0	7.5	8.3	
Chicago Commons	74	3.7	2.9	3.8	12.3	4.0	
Connelley (Pittsburgh)	184	13.0	7.2	8.9	17.0	10.0	
East LA Skills Center	100	2.5	6.6	6.8	3.8	5.4	
EGOS (Denver)	183	13.8	10.5	8.0	3.8	10.0	
Phoenix Job Corps	130	7.6	6.2	7.3	6.6	7.1	
SER/Corpus Christi	236	11.0	8.3	15.9	19.8	12.8	
Sequential/in-house							
El Centro (Dallas)	155	10.5	6.8	8.5	5.7	8.4	
LA Job Corps	218	13.6	13.2	11.2	1.9	11.9	
Sequential/brokered							
Allentown (Buffalo)	140	9.5	7.4	5.5	15.1	7.6	
BSA (NYC)	119	3.9	7.0	8.4	1.9	6.5	
CREC (Hartford)	87	5.0	6.6	3.5	3.8	4.7	
Sample size	1,839	484	484	765	106		

(continued)

100

TABLE 2.3 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a]The column labeled "p" is the statistical significance level of differences among groups in distributions of characteristics: that is, p is the probability that observed proportions in each subgroup differ from one column to another only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

CHAPTER 3

AN OVERVIEW OF JOBSTART SERVICES AND PARTICIPATION

The JOBSTART model requires sites to operate basic education and occupational skills training classes that are interesting and accessible, effective in improving the skills of young people, and of relatively long duration. It also requires young people to take advantage of these opportunities. Historically, education and training programs have had problems retaining young, economically disadvantaged dropouts (or even high school graduates).[1] Thus, a key question for the evaluation is whether youths offered JOBSTART services do actually participate in lengthy, intensive services.

This chapter looks at the JOBSTART experience from three perspectives.[2] First, it briefly summarizes the nature of program services (highlighting key aspects of site variation) and reports youths' subjective reactions to the services.[3] Second, it describes participation patterns of youths who were active in the JOBSTART Demonstration and compares that experience to other programs for young school dropouts. The analysis shows that participation was, in general, longer and more substantial than in most other JTPA Title IIA-funded activities for young dropouts, and that it was roughly comparable to participation in intensive programs such as the nonresidential Job Corps and the National Supported Work Demonstration (generally referred to simply as Supported Work).

Third, the chapter analyzes the extent to which participation varied among different groups of youths and types of sites. This analysis finds that participation hours were similar for many groups: males and females, various ethnic groups, older and younger participants, youths with relatively higher and lower levels of reading skills, and recipients and nonrecipients of public assistance. Participation hours tended to be higher in labor markets with poorer employment opportunities. Average total participation hours were highest in sites that operated sequential programs with all services provided in-house (536 hours) followed by sites with concurrent programs (405 hours) and sequential sites that referred participants to another agency for training (316 hours). Average hours in education were highest in sequential sites, while average training hours were highest at concurrent sites.

---

[1] U.S. Department of Education, 1988; Public/Private Ventures, 1988; Kelly, 1987.

[2] The chapter summarizes and updates information in Chapters 2 and 4 through 8 of Auspos et al., 1989. See that report for more details.

[3] These reactions were captured in the initial follow-up survey, which was conducted 12 months after random assignment, and in focus groups with participants. This section presents information on the JOBSTART components in specific sites. Chapter 6, which discusses program impacts by site, includes a summary of each site's program characteristics.

The discussion of site experiences reinforces three basic themes of this report. First, the variation in the details of the programs highlights the diversity of JOBSTART experiences among the sites within the general framework of the JOBSTART guidelines. Second, the experience of the sites shows that the basic program model can be implemented in a variety of administrative and labor market settings and using different basic program structures, though there were clearly stronger and weaker programs among the sites in the demonstration. Finally, this summary of how sites varied along many different dimensions provides background needed for interpreting the differences in site impacts that are presented in Chapter 6.

I. The Nature of JOBSTART Services

Basic education, occupational training, support services, and job placement assistance were available to participants in each site. To operate JOBSTART, two of the six community-based organizations (SER/Corpus Christi and Chicago Commons) added education to their regular service offerings, and three of the others (Allentown in Buffalo, BSA in New York City, and CREC in Hartford) developed or strengthened relationships with outside training programs so that they could serve as brokers, arranging training elsewhere for JOBSTART participants. The one community college (El Centro in Dallas) and three adult vocational schools (Connelley in Pittsburgh, the East Los Angeles Skills Center, and EGOS in Denver) had previously offered education and training but had to strengthen support services and job placement assistance. The three Job Corps Centers (in Atlanta, Los Angeles, and Phoenix) already had all four kinds of services in place. CET/San Jose already operated a program of integrated training and education, with support services and job placement assistance.

Table 3.1 (which groups the sites by whether they operated concurrent, sequential/in-house, or sequential/brokered programs) describes the entry and exit rules, availability of separate classes for youths, expected duration of occupational training, and scheduled hours per

TABLE 3.1

CHARACTERISTICS OF JOBSTART ACTIVITIES, BY SITE

Site	Fixed Cycle or Open Entry and Exit	Separate Classes for Youths	Expected Duration of Occupational Training	Scheduled Hours per Day			
				Education[a]	Training	Other Activities	Total
Concurrent							
Atlanta Job Corps	Open entry and exit	Yes	1 year maximum[b]	Individualized, usually 2 hours	Individualized, usually 2.5 hours at start, more in subsequent weeks	Usually 2 hours in life skills and avocational activities at start, less in subsequent weeks[c,d]	6.5 hours
CET/San Jose	Open entry and exit	In education only	600-1,000 hours during 23-37 weeks	2 hours, may vary	4.5 hours, may vary	None	6.5 hours
Chicago Commons	Fixed cycle	In education only	500-1,380 hours during 22-42 weeks	1-2 hours, 3-5 days per week	4.5-7 hours, depending on course	None	6.5-8 hours
Connelley (Pittsburgh)	Fixed cycle with semesters	Sometimes in education	700-1,000 hours	2 hours	4 hours	1 hour of counseling and other support services in school year 1986-87[c]	6 hours in school year 1985-86, 7 hours in school year 1986-87
East LA Skills Center	Open entry and exit	No	600-840 hours during 20-28 weeks	2 hours, may vary	4 hours, may vary	None	6 hours
EGOS (Denver)	Open entry and exit with semesters	In education only	600-1,000 hours	2 hours, may vary	4 hours, may vary	None[c]	6 hours

(continued)

TABLE 3.1 (continued)

Site	Fixed Cycle or Open Entry and Exit	Separate Classes for Youths	Expected Duration of Occupational Training	Scheduled Hours per Day			
				Education[a]	Training	Other Activities	Total
Phoenix Job Corps	Open entry and exit	Yes	1 year maximum[b]	Individualized, usually 2 hours	Individualized, usually 2.5 hours at start, more in subsequent weeks	Usually 2 hours in life skills and avocational activities at start, less in subsequent weeks[c,d]	6.5 hours
SER/Corpus Christi	Fixed cycle	Yes	500-660 hours during 22-23 weeks	2.5 hours for first 12-16 weeks[e]	3.5 hours for first 12-16 weeks, then 6 hours	None	6 hours
Sequential/in-house							
El Centro (Dallas)	Open entry and exit	In education only	720 hours during 24 weeks	3-4 hours	6 hours	2-3 hours in life skills activities during education phase[c,d]	6 hours
LA Job Corps	Open entry and exit	Yes	1 year maximum[b]	3 hours for first 10-12 weeks, then individualized	6 hours, may vary	3 hours in life skills or avocational activities during education phase[c,d]	6 hours
Sequential/brokered							
Allentown (Buffalo)	Open entry and exit for education, varied in training	In education only	Varied by training provider	3 hours	Varied by training provider	3 hours in life skills activities during education phase[d]	6 hours during education phase

(continued)

TABLE 3.1 (continued)

Site	Fixed Cycle or Open Entry and Exit	Separate Classes for Youths	Expected Duration of Occupational Training	Scheduled Hours per Day: Education[a]	Scheduled Hours per Day: Training	Scheduled Hours per Day: Other Activities	Scheduled Hours per Day: Total
BSA (NYC)	Open entry and exit for education, varied in training	In education only	Varied by training provider	3 hours, 4 days per week	Varied by training provider	3 hours in life skills activities during education phase, 4 days per week[d]	6 hours during education phase, 4 days per week
CREC (Hartford)	Open entry and exit for education, varied in training	No	Varied by training provider	3 hours	Varied by training provider	None[c]	3 hours during education phase

SOURCE: Adapted from Auspos et al., 1989.

NOTES: [a] Education hours refer to time spent in a basic education or GED-preparation class and do not include education provided as part of an occupational training course.

[b] Job Corps Centers offer a maximum of two years of training, but JOBSTART participants were supposed to be enrolled in courses that could be completed in one year.

[c] Some participants worked in paid or unpaid work experience positions for limited periods.

[d] Life skills classes typically provided instruction in work behaviors, goal setting, personal budgeting, health, and interpersonal relations. Avocational activities included physical education and driver education.

[e] Additional hours were available on an individualized basis after the course ended.

day in each site.[4] In some sites, participants could enter courses at any time (open entry) and leave them when they had achieved a certain competency level (open exit), while in others, they had to adhere to a fixed cycle, with entry on specified dates and exit after a set period of time. Some sites held classes for youths only, while others mixed youths and adults. Sites also varied in their expected duration of training, daily scheduling, and support services.

A. Basic Education

The education component typically consisted of individualized instruction, which allowed students to move at their own pace learning reading, mathematics, and other subjects needed to pass the GED examination. Mostly they worked on their own, doing workbook exercises or, less commonly, using computer-assisted instruction. In sites offering education and training concurrently, participants usually attended two hours of education classes and four hours of vocational training a day. In sites operating a sequential program, participants generally attended three hours a day of basic skills classes during the education phase, with the remaining three hours a day being devoted to life skills classes.

The payment provisions of the contracts between service providers and funding agencies (especially local SDAs) were an important source of variation in the emphasis of the education component. In four sites (Connelley in Pittsburgh, EGOS in Denver, El Centro in Dallas, and SER/Corpus Christi), payment for education services was based on students passing the GED examination. This led these sites to make GED certification an important short-term goal of the program and to emphasize the skills tested on the GED examination in their education component. Other sites – CET/San Jose, the East Los Angeles Skills Center, and especially Chicago Commons – saw GED attainment as a long-term goal and did not stress it in their JOBSTART programs, focusing more on improving basic skills as an aid to vocational training and job placement.

The actual curricula and instructional materials were not specified by the JOBSTART guidelines. The three Job Corps sites used the standard Job Corps materials (workbooks, textbooks, and audiovisual materials), though two centers (Atlanta and Phoenix) also had supplementary computer-assisted instruction. The three sequential/brokered sites used the Comprehensive Competencies Program (CCP) developed by U.S. Basic Skills Investment

---

[4] This grouping was chosen because, as discussed later, participation rates by component, participation hours, and program emphasis differed among these three types of sites.

Corporation. CCP is an instructional management system integrating textbooks, workbooks, computer software, audiovisual materials, and progress tests. In the seven other sites, teachers developed their own instructional materials using a variety of sources, such as GED preparation courses and reading and mathematics textbooks that used the "mastery learning" approach, which focuses on the step-by-step acquisition of specific competencies. In four of these sites, staff supplemented pencil and paper exercises with computer-assisted instruction.[5]

Teachers in most sites felt that the individualized, self-paced instruction provided a better learning environment than participants had typically found in high school. The competency-based courses allowed the youths to see themselves making incremental progress as they advanced toward what was, for many, a remote goal of mastering basic skills and receiving a GED. Most students preferred this instructional approach because they felt that it made them active participants in the process of learning and allowed them to master one topic before beginning another. In the follow-up survey, about three-fourths of JOBSTART participants found self-paced instruction "very helpful," while virtually no one found it "not helpful at all."

Yet students also valued interaction with instructors, as much for the personal attention and motivation it provided as for instruction in specific skills. About 75 percent of JOBSTART participants rated support from teachers and fellow students in the education component "very helpful."

Despite the overall favorable assessment, three concerns emerged. First, with a few important exceptions, the basic education and skills training activities operated separately, with little integration of material. As discussed in Chapter 1, only at CET/San Jose, and to a lesser extent Chicago Commons, were basic skills and occupational training instruction truly integrated. Though several other sites did attempt to coordinate the two activities to a limited extent (creating a distribution of sites rather than two clear-cut categories), these sites fell short of the integration observed at CET/ San Jose and Chicago Commons. Second, some instructors thought the curriculum should include more material on critical thinking and general knowledge, in contrast to the functional literacy and mathematics emphasis of many integrated programs. Third, some instructors said that students with very poor skills or low motivation found the work boring and, as a remedy, suggested more group activities. One site, El Centro in Dallas, shifted to this approach, relying more heavily than other sites on class exercises and lectures.

---

[5] See Auspos et al., 1989, for the details of these programs.

B. Occupational Skills Training

The choices of occupational training available to participants varied among the sites. Participants at large vocational schools could choose courses in more than 20 occupational areas. The Job Corps Centers and larger community-based organizations (CBOs) also offered a wide range of vocational training. In contrast, SER/Corpus Christi, which provided training in-house, offered only a few courses.

In theory, youths in sequential/brokered sites could choose courses from a variety of local agencies. However, in practice, some courses were unavailable to them because they could not satisfy entrance requirements, or other difficulties prevented them from gaining entry.[6] As discussed later in this chapter, the resulting low rate of participation in training in sequential/brokered sites was the major operational issue concerning the training component.

As a group, JOBSTART participants were enrolled in training for a broad range of occupations – clerical and service jobs, machine trades, benchwork occupations, and structural work such as welding. Occupational choices for men and women followed traditional patterns, as shown in Table 3.2, with about three-fourths of the women participants training for clerical jobs.[7]

Using categories employed by the U.S. General Accounting Office (GAO) in a recent analysis of JTPA Title IIA adult training, MDRC classified the JOBSTART training provided to participants as leading to jobs requiring low or low/moderate skills (slightly less than one-fourth of participants), moderate skills (about one-half of participants), and higher skills (about one-fourth of participants).[8] This distribution of skills ratings for training occupations was similar to what the GAO found for JTPA Title IIA adult programs. This was unexpected,

---

[6] As mentioned in Chapter 1, the JTPA performance standards created an incentive for SDAs to emphasize lower-cost, short-term programs. Some SDAs in study sites were reluctant to provide a single individual with both education and training, and many JTPA Title IIA-funded service providers operated under performance-based contracts linking payment to placement in a job. Both practices hindered the efforts of JOBSTART youths in sequential/brokered sites to find a training agency willing to accept them.

[7] This table, taken from an earlier report on JOBSTART (Auspos et al., 1989), is based on a similar, but slightly smaller, sample than that used for this report.

[8] See U.S. General Accounting Office, 1988, for the definitions of categories of training. The percentage distribution reported in the text for JOBSTART was calculated in Auspos et al., 1989, based on a slightly different sample of participants than that used in this report.

TABLE 3.2

PERCENTAGE DISTRIBUTION OF OCCUPATIONS FOR PARTICIPANTS IN TRAINING, BY GENDER

Training Category[a]	Men	Women	Total
Clerical and sales occupations			
Stenography, typing, filing, and related occupations	5.0%	51.3%	29.2%
Computing and account-recording	7.0	20.3	13.9
Production and stock clerks, and related occupations	0.3	0.0	0.1
Information and message distribution	1.4	0.0	0.7
Miscellaneous clerical	0.0	0.5	0.3
Sales and consumable commodities	0.8	1.0	0.9
Total	14.6	73.1	45.1
Service occupations			
Food and beverage preparation and services	3.1	4.6	3.9
Miscellaneous personal services	0.6	11.5	6.3
Building and related services	8.4	2.1	5.1
Total	12.0	18.2	15.3
Machine trades occupations			
Metal machining	5.0	0.8	2.8
Mechanics and machinery repair	22.4	1.3	11.4
Printing	0.6	1.3	0.9
Wood machining	0.6	0.5	0.5
Total	28.6	3.8	15.7
Benchwork occupations			
Assembly and repair of electrical equipment	11.5	1.3	6.2
Painting, decorating, and related occupations	0.8	0.3	0.5
Fabrication and repair of plastics, synthetics, rubber, and related products	2.2	0.3	1.2
Fabrication and repair of textile, leather, and related products	1.7	0.3	0.9
Total	16.2	2.1	8.8
Structural work occupations			
Metal fabricating	9.8	0.5	5.0
Welders, cutters, and related occupations	0.8	0.0	0.4
Electrical assembling, installing, and repairing	5.9	0.5	3.1
Painting, plastering, waterproofing, cementing, and related occupations	1.7	0.0	0.8
Construction	8.4	1.3	4.7
Total	26.6	2.3	13.9
Miscellaneous occupations			
Transportation	0.0	0.3	0.1
Graphic art work	2.0	0.3	1.1
Total	2.0	0.5	1.2
All training categories	100.0	100.0	100.0
Number of participants in training	357	390	747

(continued)

TABLE 3.2 (continued)

SOURCE: Adapted from Auspos et al., 1989. The categorization of occupations is derived from U.S. Department of Labor, 1977.

NOTES: Calculations for this table used data for all youths who were active for at least one hour in a JOBSTART training component within 12 months of random assignment and responded to the 12-month follow-up survey.

Distributions may not total 100.0 percent because of rounding.

Individual category totals may not equal the general category totals because of rounding.

Tests of statistical significance were not run.

[a] Individuals participating in more than one training category are included in the category in which they attended the most hours.

since JOBSTART participants faced more barriers to employment than did the typical JTPA Title IIA adult client.

One argument for sequential programs is that the up-front education allows participants to enter more advanced training. In the JOBSTART Demonstration, this did not appear to occur. In terms of the same GAO categories, the jobs that youths trained for in sequential sites did not appear to require higher skills than those in concurrent sites.

C. Support Services To Facilitate Participation

All sites provided basic support services such as assistance with transportation and child care, which helped participants attend the program, as shown in Table 3.3. All sites provided bus passes or small allowances to cover the costs of commuting to the program. JOBSTART counselor/coordinators placed a high priority on adequate child care arrangements. In most sites, staff referred JOBSTART participants to other agencies for child care, with the expenses being covered by JTPA or the Work Incentive (WIN) program. The Atlanta Job Corps, two CBOs (SER/Corpus Christi and CET/San Jose), and one adult school (Connelley in Pittsburgh) had on-site day care facilities, but staff reported that students frequently preferred to make their own arrangements in their own neighborhoods.

Many sites also found ways to provide small payments to meet other costs of participating, though the Job Corps Centers were consistently able to provide more support than the other sites. Ten of the 13 sites were able to provide some type of small needs-based payment, while nine of the sites provided on-site meals or food to take home or special allowances for clothing or to meet rent emergencies. Seven of the sites (including all three Job Corps Centers) provided some form of incentive payments to participants who reached milestones in the program. The Job Corps Centers also provided on-site medical and dental care.

In addition, to increase participants' motivation and commitment to the program, site staff used a variety of strategies: personal counseling, peer support, time management training, and group recreational activities. Finally, staff at most sites provided training in life skills – covering topics such as health, personal finances, and workplace routines – to help the young people function more responsibly and productively in a variety of roles and situations. Six of the sites (the three Job Corps programs, El Centro in Dallas, Allentown in Buffalo, and BSA in New York City) incorporated two to three hours of formal life skills classes into the regular

TABLE 3.3

BASIC SUPPORT SERVICES AVAILABLE IN JOBSTART, BY SITE

Site	Needs-Based Payments	Transportation	Child Care	Other	Incentive Payments
Job Corps Centers					
Atlanta Job Corps	Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months	Bus passes	On-site	Free meals; clothing allowance of $75 in first month, $50 in third month, $96 in sixth and tenth months, $51 in twelfth month; on-site medical and dental care	Merit raises can increase basic allowance to $100 per month after 6 months; $75 per month is placed in escrow for enrollees who stay 6 months, which increases to $100 per month after 6 months; $150 bonus in tenth month
LA Job Corps	Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months	Bus passes	By referral	Free meals; clothing allowance of $75 in first month, $50 in third month, $96 in sixth and tenth months, $51 in twelfth month; on-site medical and dental care	Merit raises can increase basic allowance to $100 per month after 6 months; $75 per month is placed in escrow for enrollees who stay 6 months, which increases to $100 per month after 6 months; $150 bonus in tenth month
Phoenix Job Corps	Basic allowance of $40 per month for first 2 months, $60 for next 3 months, $80 after 5 months	Bus passes	By referral	Free meals; clothing allowance of $75 in first month, $50 in third month, $96 in sixth and tenth months, $51 in twelfth month; on-site medical and dental care	Merit raises can increase basic allowance to $100 per month after 6 months; $75 per month is placed in escrow for enrollees who stay 6 months, which increases to $100 per month after 6 months; $150 bonus in tenth month
Schools					
Connelley (Pittsburgh)	$5 per day[a]	$2 per day or bus passes[a]	On-site and by referral	$50 one-time clothing grant	$50 for passing GED;[b] $50 for each month of perfect attendance; quarterly payment of $50 for "A" average, $25 for "B" average, $10 for "C" average

(continued)

TABLE 3.3 (continued)

Site	Needs-Based Payments	Transportation	Child Care	Other	Incentive Payments
East LA Skills Center	None	Bus passes, gasoline vouchers	By referral	Emergency funds, lunch money during a brief period	None
EGOS (Denver)	None	Bus passes, gasoline vouchers	By referral	Lunch money during a brief period	None
El Centro (Dallas)	$5 per day	Bus passes	By referral	Emergency rent funds	$5 per week for perfect attendance
Community-based organizations					
Allentown (Buffalo)	$1 per hour if on AFDC, otherwise $2 per hour, during education and training	Included in needs-based payment	By referral	None	None
BSA (NYC)	$23-$30 per week during education,[c] $30 per week during JTPA training	Included in needs-based payment; tokens available otherwise	By referral, $15 per week for expenses	Free breakfasts	$5 for weekly academic progress; $5 for perfect weekly attendance[d]
CET/San Jose	$1 per hour, for farmworkers only	Bus passes for farmworkers and others who demonstrate need	On-site and by referral	Weekly food bank to provide free groceries	None
Chicago Commons	$6 per day	Included in needs-based payment	By referral	None	None
CREC (Hartford)	None	Bus passes	By referral	None	None
SER/Corpus Christi	$8 per day	Included in needs-based payment	On-site for children over 18 months and by referral	None	$20 for each grade-level gain in reading; $20 for passing GED pre-test; $40 for passing GED test; $45 for "A" average throughout occupational training, $25 for "B" average

SOURCE: Adapted from Auspos et al., 1989.

NOTES: [a] At intervals, this site combined transportation and needs-based payments into one $7 per day payment.
[b] Available during 1986-87 school year.
[c] During October 1986-August 1987.
[d] Available after October 1987.

program day.[9] The remaining seven sites did not focus as systematically on life skills, instead incorporating these topics into the training curriculum, counseling or group discussion sessions, or occasional lectures.

Youths cited personal attention from staff as a crucial aid in helping them move toward self-sufficiency. While agencies that traditionally served disadvantaged youths typically offered these support services from the beginning of the demonstration, a number of sites accustomed to serving adults increased this type of activity as their programs evolved.

Clearly, the support services and other activities available at the Job Corps Centers surpassed those at the other sites in both breadth and intensity. To a large extent, the JOBSTART participants in these sites were able to partake of the full array of Job Corps activities, including recreational, health, and food services. However, Allentown in Buffalo, Connelley in Pittsburgh, and SER/Corpus Christi also offered high levels of these services.

D. Job Placement Services

Sites were required to assist youths in finding training-related employment, but this phase of the program typically received less attention than others. Nearly all the sites did provide instruction about employers' expectations as well as job search techniques. About one-half of the sites arranged paid or unpaid part-time work experience positions for some participants during the program. Approximately one-fourth of a sample of participants worked at some point – in program-arranged or self-initiated jobs – while they were active in the program. Those in the sample who were employed worked an average of about 50 percent of the weeks they were in the JOBSTART program and were employed an average of about 30 hours per week during the weeks they worked. During the months they worked, their hours of classes in JOBSTART were lower than were those of nonworking participants.

Efforts to find participants permanent employment typically began near the end of training, with instructor contacts serving as an important source of information about job openings. Since many youths left the program without reaching this stage, it is not surprising that only about one-fourth of participants reported that program staff referred them to a job or told them about openings.

---

[9] The life skills curricula in these sites was oriented around daily living, with units on health education, substance abuse prevention, sexuality and family planning, personal finances, civics, communication skills, goal-setting and planning, and improving one's self-esteem.

Job placement assistance was especially strong at CET/San Jose, Chicago Commons, and the Job Corps program in Phoenix. In all these sites, instruction in proper work behavior, employer expectations, and job search techniques began while students were still in training; placement specialists provided leads and assistance in finding a job; and CET/San Jose and Chicago Commons had especially strong ties to local employers.

Job placement assistance was noticeably weaker at Allentown in Buffalo, BSA in New York City, and CREC in Hartford (the three sequential/brokered sites), and at the Atlanta Job Corps, the East Los Angeles Skills Center, EGOS in Denver, and SER/Corpus Christi (among the concurrent sites). At the first three of these sites, job placement was intended to be the responsibility of the training agency, but most participants never were active in that component so only informal assistance was available from the JOBSTART agency. Those concurrent sites with weak job placement typically lacked any or sufficient job development specialists on staff, were larger agencies with no special emphasis on placing JOBSTART youths, or contracted out job placement to another organization that did not see the JOBSTART youths as a high-priority group.

E. Scheduling, Daily Service Mix, and Planned Program Duration

Sites also varied in the way they scheduled classes and the expected duration of their programs. The demonstration sites scheduled JOBSTART classes in three basic ways. The majority of sites that operated both the education and training components themselves scheduled the classes on an "open-entry/open-exit" basis.[10] This means that participants could enter the program at any time, progress through the material at their own pace, and complete the course whenever they reached the specified competency levels. The duration of training was open-ended, but sites anticipated that participants would typically be able to complete the prescribed training curriculum in many fields in approximately 600 to 800 hours. Individuals who needed additional time to complete competencies could stay longer, however.

In a second program variation, some concurrent sites operated JOBSTART as a series of "fixed cycles," meaning that all participants started and completed training together on specified dates and the maximum length of training was prescribed.[11] In a third variation, the three sequential/brokered sites operated the education component on an open-entry/open-exit schedule, but the training schedule was determined by the variety of training organizations at which JOBSTART participants were enrolled.

---

[10] These sites included concurrent sites (the Atlanta Job Corps, CET/San Jose, the East Los Angeles Skills Center, and the Phoenix Jobs Corps) and sequential/in-house sites (El Centro in Dallas and the Los Angeles Job Corps).

Sites also showed great variety in the number of hours scheduled for activities each day. The usual schedule ranged from a low of three hours per day at CREC in Hartford to seven to eight hours per day in some courses at Chicago Commons. A typical day can be described in terms of three basic models:

- **Concurrent sites that were CBOs or schools.** Students typically had six hours of classes per day, five days a week. In general, two hours were spent in education classes, with training classes scheduled for the remaining four hours.
- **Concurrent sites that were Job Corps Centers.** These sites had six and a half class hours per day. Schedules were highly individualized and changed frequently, but commonly included two hours of education, two and a half hours of vocational training, and two hours devoted to life skills, health education, or avocational activities such as sports.
- **Sequential sites.** These also scheduled a six-hour day during the education phase, but the daily distribution of activities was quite different. Typically, three hours were spent in education classes and another three hours were spent in life skills training. The training schedules were set by the training providers at the brokered sites, but typically involved five to six hours of classes per day. Training classes ran for six hours a day at the sequential/in-house sites.

The duration of the occupational training component also varied among the sites, ranging from 22 to 23 weeks at SER/Corpus Christi to a year at the Job Corps sites. Even within a site, there could be significant variation among the different training options. At Chicago Commons, for example, scheduled training ranged from 500 hours in industrial inspection to 1,380 hours in packaging-machine repair.

This diversity in scheduled daily hours and program duration meant that the planned participation hours for youths varied greatly across the sites, with the greatest variation showing in the training component. At SER/Corpus Christi, a participant completing education and training in about six months, as planned, would have had no more than 660 hours of occupational training. In contrast, one training course at Chicago Commons totaled nearly 1,400 hours, and a sequential program such as the one operated by the Los Angeles Job Corps could last for as long as a year.[12]

---

[11] These sites included Chicago Commons, Connelley in Pittsburgh, EGOS in Denver, and SER/Corpus Christi.

F. Summary of Program Implementation by Site

Table 3.4 rates the implementation of the four central JOBSTART components in each site. (See Appendix Table D.1 for the details behind these ratings.) The information in Table 3.4 and the material already presented in this section suggest that the four key components of the JOBSTART program were implemented most successfully at CET/San Jose,[13] Chicago Commons, Connelley in Pittsburgh, El Centro in Dallas, the Los Angeles Job Corps, and the Phoenix Job Corps and least successfully at the Atlanta Job Corps, BSA in New York City, CREC in Hartford, and EGOS in Denver. To summarize the ratings by component:

- **Education.** Most sites that chose to operate a separate education component were able to offer an activity meeting the JOBSTART guidelines. The two sites with noticeably weak education activities were the Atlanta Job Corps (where unclear objectives for education and staff turnover hampered implementation) and CREC in Hartford (where computer facilities were under-utilized and poor attendance was a serious problem).

- **Training.** The training component showed the most variation – primarily because in sequential/brokered sites most youths never participated in training. In addition, the limited training offerings, less experienced staff, and older equipment of SER/Corpus Christi (reflecting the common problems of a community-based organization) hampered its ability to implement the training component.

- **Support services.** Although the Job Corps Centers did offer substantially more services than other sites, all programs were able to provide the limited types of support services called for in the JOBSTART guidelines: assistance in arranging and/or financing child care and transportation to and from coursework. Allentown in Buffalo, Connelley in Pittsburgh, and SER/Corpus Christi, in addition to the three Job Corps Centers, provided a noticeably longer list of services, including better needs-based

---

[12] Job Corps Centers offer a maximum of two years of training, but JOBSTART participants were only to be enrolled in courses that could be completed in one year.

[13] CET/San Jose provided most of its basic education services within training activities, so this overall characterization reflects a judgment about the other three components and the way in which education was incorporated into the training component.

TABLE 3.4

RATINGS OF THE IMPLEMENTATION OF JOBSTART COMPONENTS, BY SITE

Site	Education	Training	Support Services	Job Placement
Concurrent				
Atlanta Job Corps	Medium	Medium	High	Low
CET/San Jose	No rating[a]	High	Medium	High
Chicago Commons	Medium	High	Medium	High
Connelley (Pittsburgh)	High	High	High	Medium
East LA Skills Center	Medium	Medium	Medium	Low
EGOS (Denver)	Medium	Medium	Medium	Low
Phoenix Job Corps	Medium	High	High	High
SER/Corpus Christi	High	Low	High	Low
Sequential/in-house				
El Centro (Dallas)	High	Medium	Medium	Medium
LA Job Corps	Medium	Medium	High	Medium
Sequential/brokered				
Allentown (Buffalo)	High	Low	High	Low
BSA (NYC)	Medium	Low	Medium	Low
CREC (Hartford)	Low	Low	Medium	Low

SOURCE: MDRC operations staff.

NOTES: See Appendix Table D.1 for details of the implementation of components in each site.

[a] In this site, a separate rating of the education component was inappropriate because education and training were more integrated than in other sites and staff strongly emphasized training over passing the GED examination.

payments, life skills training and counseling, and a method of identifying service needs and making referrals of youths to other agencies providing the required services.

- **Job placement assistance.** Most sites offered job placement assistance that fell short of the JOBSTART guidelines, either because many youths never received the service (especially in sequential/brokered sites) or too few staff with a specialty in job search assistance were assigned to work with the JOBSTART youths. As mentioned earlier, job placement assistance was especially strong at CET/San Jose, Chicago Commons, and the Phoenix Job Corps.

II. The Intensity of JOBSTART Participation

Participation in JOBSTART among experimentals was measured by participation rates in each activity, hours of participation in each activity, and overall length of stay. Table 3.5 shows these summary measures for all experimentals in the impact sample:

- **Participation rates.** Nearly 90 percent of all experimentals in the impact sample participated in JOBSTART to some extent. Eighty-six percent of all experimentals (and nearly all of those who were active in JOBSTART) attended basic skills education classes, while 67 percent participated in training, and 40 percent participated in other activities, which were optional for sites.

- **Participation hours.** Average hours were 128 in education, 249 in training, and 37 in other activities, for a total of 415 hours.[14] Forty percent of all experimentals spent fewer than 201 hours in all JOBSTART activities; 26 percent spent 201 to 500 hours, and 35 percent spent more than 500 hours.

- **Length of stay.** The average length of stay was 6.8 months, with the median length being slightly less, 6 months; 78 percent of experimentals were active for 3 months or more, while 54 percent stayed in the program for 6 months or more. This was measured from the time of random assignment through the last month that included any hours of participation.[15] Sixteen percent of the experimental sample were still

---

[14] These averages and those cited in the next paragraph include the 11 percent of the sample with zero hours of JOBSTART activities.

[15] The period of participation could include months of inactivity if a person stopped attending classes and then returned to the program within the 12-month follow-up period. However, this does not appear to have been a common pattern. Among a sample of participants, about 85 percent did not have any months of inactivity within the period they were counted as active, and among those with inactivity, the average period of inactivity was about two months. Youths who attended JOBSTART were counted as (continued...)

TABLE 3.5

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS

Activity Measure	Experimentals
Percent participating in	
Any activity	88.7
Education	85.9
Training	66.6
Education and training	64.4
Other activities	40.0
Average hours in	
Education	128.1
Training	248.9
Education and training	377.0
Other activities	37.3
All activities	414.8
Percentage distribution of hours in education and training	
None	11.9
Up to 200	33.2
201 to 500	22.4
501 to 700	15.5
701 or more	17.0
Total	100.0
Percentage distribution of hours in all activities	
None	11.3
Up to 200	28.6
201 to 500	25.5
501 to 700	15.2
701 or more	19.5
Total	100.1
Length of stay (months)	
Average	6.8
Median	6.0
Percent still participating in month	
3	78.0
6	53.6
9	30.6
12	16.4
15	9.6
18	4.8
19 or later	3.7
Number of experimentals	949

(continued)

TABLE 3.5 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

> active in the program in the twelfth month after random assignment, while 10 percent were still active in the fifteenth month.

These findings show that JOBSTART succeeded in engaging more than half of the youths in the experimental group in the program and its activities, but that for about 40 percent of them, participation was quite low and JOBSTART did not constitute an intensive or lengthy program. Because of this wide range of participation levels, with some people having very few hours, the average total hours for the sample as a whole is the equivalent of slightly less than three and one-half months of regular attendance for six hours per day. Most people in the sample did not participate long enough to get a GED or complete a training course.

To place these results in context, JOBSTART participation may be compared to reported participation in other programs for young, disadvantaged school dropouts. Length of participation is a simple measure that permits comparisons with three types of youth programs: JTPA Title IIA programs for young dropouts, the Job Corps, and Supported Work.[16] JTPA Title IIA typically provides relatively short-term activities, while the Job Corps and Supported Work have been among the most intensive employment and training programs for disadvantaged youths. In these comparisons, either the average or median length of participation is used, depending on the available data.

Overall, JOBSTART participants stayed in the program considerably longer than did young dropouts in JTPA Title IIA activities, as shown in Table 3.6. During program year 1986, when the demonstration was in operation, the median length of participation for all young dropouts in JTPA Title IIA programs was 3.4 months compared to 6 months for JOBSTART.[17] JOBSTART's median length of participation exceeded that of young dropouts

---

[15] (...continued)
participating for the entire month in which they were randomly assigned and all months in which they showed any JOBSTART hours. The measure might have overestimated the length of participation somewhat when a youth was randomly assigned late in a month or ended participation early in a month.

[16] For information on the Job Corps, see Richardson and Burghardt, 1985, and U.S. Department of Labor, 1987. On Supported Work, see Maynard, 1980. For JTPA Title IIA, see U.S. Department of Labor, 1988. Hours of attendance are not reported for all programs, hence this comparison uses length of stay, for which the data are available.

[17] The average length of participation in JTPA Title IIA programs is not available from published sources. The figure for JTPA Title IIA includes only persons who actually participated, while the JOBSTART figure includes the 11 percent of the sample made up of nonparticipants with zero months of activity.

TABLE 3.6

PARTICIPATION AND LENGTH OF STAY FOR YOUNG DROPOUTS IN JTPA TITLE IIA, BY ACTIVITY

Activity	Percentage Distribution of Youths in JTPA	Median Length of Stay (Months)
Classroom activities		
Basic education	22.8	3.71
Occupational skills training	15.6	3.98
Combined basic education and occupational skills training[a]	4.6	6.97
Total	42.9	3.97
On-the-job training	12.2	3.14
Job search assistance	15.3	0.81
Work experience	7.8	3.67
Other services	21.8	3.59
Any activity	100.0	3.40

SOURCE: U.S. Department of Labor, Division of Performance Management and Evaluation, 1988.

NOTES: This table includes data for young dropouts served under JTPA Title IIA during program year 1986.

[a] JTPA data (as recorded by the U.S. Department of Labor, Division of Performance Management and Evaluation, 1988) combined basic education and occupational skills training under the label CT-Other.

in all JTPA components except one. The exception was a program combining basic education and occupational skills training, a mix similar to JOBSTART's, which had a median length of 7 months but was offered to only 5 percent of all young dropouts in JTPA Title IIA activities. For JOBSTART participants active in both education and skills training, the median length of stay in the program was also approximately 7 months. These findings support the conclusion that JOBSTART achieved its goal of operating a program more intensive than that typically offered in JTPA Title IIA programs for young dropouts.

JOBSTART's average length of participation was similar to those of the Job Corps and Supported Work. During program year 1986, the average stay in the Job Corps was 6.9 months, compared to JOBSTART's average of 6.8 months.[18] Supported Work was an experimental program of *paid* work experience under conditions of gradually increasing responsibility on the job, close supervision, and work in association with a crew of peers. It operated from 1975 to 1979 and included young school dropouts, many with a criminal record, as one of its target groups. While precise comparisons are impossible, the length of participation in the two programs appears to have been similar.[19] The average length of participation in Supported Work was 6.7 months (close to that in JOBSTART) and the median was approximately 6 months (the same as in JOBSTART), but 25 percent of Supported Work participants were still active in the program at 12 months after random assignment, as opposed to 16 percent for JOBSTART.

In summary, while only approximate comparisons can be made, it appears that JOBSTART achieved its goal of providing young school dropouts with more intensive education and training than is usual within the JTPA system. The data also suggest that JOBSTART offered an intensity of activity close to that of the Job Corps and Supported Work, which operated through special agencies and had the sole mission of providing services to very disadvantaged individuals. However, for the 40 percent of the JOBSTART sample who did not participate or had very few hours of activity, the treatment was unlikely to be intense and lengthy enough to move them above the threshold of skills needed to secure significantly better jobs than they could before the program.

---

[18] The median for the Job Corps is not available.

[19] The JOBSTART measure of length of participation included some periods of inactivity in the midst of participation, while the Supported Work measure factored these out. As discussed above, however, this problem does not appear to have been serious in the JOBSTART data.

III. Moving Behind the Aggregate Participation Measures

Aggregate measures, however, tell only part of the story. Table 3.5 makes clear that JOBSTART was not the same experience for all youths: 40 percent participated for 200 or fewer total hours, while nearly 20 percent exceeded 700 hours, the required offering under the demonstration (200 hours of education and 500 hours of training). Clear differences in average participation also existed among the sites, as discussed later in this chapter. Understanding the sources of these variations in participation is the first step in developing ways to improve the design and implementation of the program.

The following analysis begins with subgroups of JOBSTART participants. It shows that while there were differences among subgroups, they did not seem to account for all the variation in participation. This implies that factors such as unmeasured differences among youths, local employment opportunities, and program characteristics associated with particular sites may also have affected participation.

The key finding on program characteristics is that youths in sites operating sequential/brokered programs tended to have lower rates of participation in occupational skills training, although they tended to receive more intensive instruction in basic skills.

A. Differences in Participation Among Subgroups

Although JOBSTART participants all satisfied the program's eligibility requirements, they varied in gender, age, marital and parental status, criminal records, and educational attainment, among other characteristics. Research and operational experience suggest that these types of factors can influence participation in programs.[20]

Among JOBSTART participants, two groups are of special concern: males (who have often been hard to recruit and retain in programs) and young mothers (a group at risk of long-term welfare receipt). As Table 3.7 shows, average total hours and other measures of participation were similar for all males and females, although a higher percentage of females were active in the twelfth month after random assignment. There were some differences, however, for females living with their children, compared to other women: Mothers averaged

---

[20] See, for example, Public/Private Ventures, 1988, and Mathematica Policy Research, 1985.

TABLE 3.7

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY, BY GENDER AND PARENTAL STATUS

		Women			
Activity Measure	Men	Living with Own Child(ren)	Not Living with Own Child(ren)[a]	All Women	Men and Women
Percent participating in					
Any activity	89.0	89.6	87.4	88.5	88.7
Education	85.8	87.6	84.3	85.9	85.9
Training	67.4	69.6	62.5	65.9	66.6
Education and training	64.6	68.4	60.2	64.2*	64.4
Other activities	36.1	41.6	45.2	43.4	40.0**
Average hours in					
Education	120.2	119.6	149.4	134.8**	128.1
Training	258.5	250.5	231.4	240.7	248.9
Education and training	378.7	370.1	380.8	375.6	377.0
Other activities	31.6	36.2	47.8	42.1**	37.3***
All activities	410.3	408.1	428.5	418.5	414.8
Percentage distribution of hours in education and training					
None	11.4	11.2	13.4	12.3	11.9
Up to 200	31.7	36.8	32.2	34.4	33.2
201 to 500	24.0	19.2	23.0	21.1	22.4
501 to 700	16.7	16.0	13.0	14.5	15.5
701 or more	16.2	16.8	18.4	17.6	17.0
Total	100.0	100.0	100.0	99.9	100.0
Percentage distribution of hours in all activities					
None	11.0	10.4	12.6	11.5	11.3
Up to 200	27.6	30.8	28.0	29.4	28.6
201 to 500	26.7	24.8	24.1	24.5	25.5
501 to 700	16.2	15.6	13.0	14.3	15.2
701 or more	18.5	18.4	22.2	20.4	19.5
Total	100.0	100.0	99.9	100.1	100.1
Average length of stay (months)	6.4	7.2	7.1	7.2	6.8**
Percent still participating in month					
3	77.6	79.2	77.4	78.3	78.0***
6	52.5	55.6	53.6	54.6	53.6
9	27.4	33.2	33.3	33.3	30.6
12	11.4	20.4	21.1	20.7	16.4
15	7.5	11.6	11.1	11.4	9.6
18	3.4	6.8	5.4	6.1	4.8
19 or later	2.7	4.4	4.6	4.5	3.7
Number of experimentals	438	250	261	511	949

(continued)

TABLE 3.7 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Among all women, the distributions compared are those for women who were living with their own child(ren) and those for women not living with their own child(ren), including those who were childless, at the time of random assignment. An F-statistic was used to test the hypothesis of equal column means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] Includes women who did not have children.

somewhat fewer hours of participation, and a slightly higher percentage received fewer than 200 hours of services.

Table 3.8 presents average total hours of participation in JOBSTART for several other subgroups. Although past research and experience suggest that the characteristics listed in the table might affect participation, many of the comparisons do not show significant differences in average hours for the groups under review. Youths who had not been arrested since age 16 participated for significantly more hours.[21] But other groupings did not show differences in hours.

B. Differences in Participation Among Sites

Hours of participation in the sites in the demonstration varied considerably, as shown in Table 3.9. Average total hours ranged from a high of 631 for experimentals at the Los Angeles Job Corps to a low of 166 at CREC in Hartford, a spread of 465 hours. Another important aspect of variation was the percentage of experimentals still participating in JOBSTART at 12 months after random assignment. This proportion varied from a low of zero percent at Chicago Commons and SER/Corpus Christi to a high of 42 percent at Allentown. The proportion still active at 18 months ranged from zero percent to 17 percent at the Los Angeles Job Corps. Thus, the 24 months of follow-up do not represent the same length of *post-program* follow-up at all sites.

As noted earlier, this variation could have had several possible sources, such as characteristics of the youths, local employment opportunities, and program characteristics.[22] With only 13 sites in the demonstration, it is very difficult to isolate the effects on participation of the many differences among programs. If, for example, the sites with the most support services were also Job Corps sites and also operated a youths-only program, it would be impossible to separate out the effects of these individual factors on participation hours.

---

[21] The mix of activities did differ by initial reading score. Those testing in the low group averaged slightly more hours in education, and had somewhat fewer hours in training, than those in the highest group.

[22] Differences in the way random assignment interacted with site recruitment efforts, and also in attendance reporting, led to variations in participation rates as well. At CET/San Jose, for example, services were not available for the first part of the sample for up to one month. Furthermore, as discussed in Chapter 2, attendance at a multi-day assessment of occupational training interests was not included in reported hours. As a result, 36 percent of the experimentals at that site had no reported hours in program services. At other sites, the gap between random assignment and reported program start-up was shorter and participation rates were higher.

TABLE 3.8

AVERAGE TOTAL PARTICIPATION HOURS, BY SELECTED CHARACTERISTICS OF EXPERIMENTALS AT THE TIME OF RANDOM ASSIGNMENT

Characteristic and Subgroups	Average Total Hours	Number of Experimentals
Age		
16-19	405.1	703
20 or 21	442.3	246
Ethnicity[a]		
White, non-Hispanic	401.7	72
Black, non-Hispanic	403.7	439
Hispanic	393.5	411
School grade at time of dropout		
Grade 10 or below	423.6	548
Grade 11 or 12	402.6	401
Reading grade level		
1-6	364.1	258
7-8	370.7	195
9 or above	302.3	18
Gender		
Women	418.5	511
Men	410.3	438
Marital status		
Ever married	439.8	88
Never married	413.2	857
Parental status		
Women living with own child(ren)	408.1	250
Women not living with own child(ren)[b]	428.5	261
AFDC benefits received		
None	409.9	438
Own AFDC case	448.8	197
Household AFDC case	431.7	186
Received occupational training within past year		
No	420.0	797
Yes	387.3	152
Criminal record		
No arrest since age 16	435.7	805
Arrested since age 16	297.8***	144
Sample size		949

(continued)

TABLE 3.8 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all 949 experimentals for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

An F-statistic was used to test the hypothesis of equal means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] The sample also included 27 experimentals who were members of other ethnic groups.

[b] Includes women who did not have children.

TABLE 3.9

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY FOR EXPERIMENTALS, BY SITE

Activity Measure	Concurrent								Sequential/ In-House		Sequential/ Brokered			Total
	Atlanta Job Corps	CET/ San Jose	Chicago Commons	Connelley (Pittsburgh)	East LA Skills Center	EGOS (Denver)	Phoenix Job Corps	SER/ Corpus Christi	El Centro (Dallas)	LA Job Corps	Allentown (Buffalo)	BSA (NYC)	CREC (Hartford)	
Percent participating in														
Education	81.8	44.0	81.1	96.8	82.4	93.5	83.6	98.3	100.0	77.4	100.0	73.8	88.9	85.9***
Training	78.8	60.0	91.9	98.9	82.4	77.4	83.6	98.3	48.1	53.0	33.8	23.1	15.6	66.6***
Education and training	78.8	40.0	81.1	96.8	82.4	77.4	83.6	98.3	48.1	53.0	33.8	23.1	15.6	64.4***
Other activities	84.8	0.0	0.0	0.0	0.0	0.0	79.1	0.0	100.0	79.1	95.9	75.4	15.6	40.0***
Average hours in														
Education	95.1	28.8	72.1	105.1	73.4	126.0	163.3	123.9	141.8	158.6	244.3	144.9	118.5	128.1***
Training	149.8	336.9	372.8	473.4	272.0	147.8	218.1	294.2	179.0	362.4	113.1	66.1	36.3	248.9***
Education and training	244.9	365.7	444.9	578.7	345.4	273.8	381.4	418.1	320.8	521.1	357.4	211.0	154.8	377.0***
Other activities	50.7	0.0	0.0	0.0	0.0	0.0	59.7[a]	0.0	80.2	105.6	82.2	69.0	10.7	37.3***
All activities	295.7	365.7	444.9	578.7	345.4	273.8	441.2	418.1	401.0	630.5	439.6	280.1	165.5	414.8***
Percentage distribution of hours in education and training														
None	18.2	36.0	8.1	1.1	17.6	6.5	16.4	1.7	0.0	22.6	0.0	26.2	11.1	11.9
Up to 200	42.4	16.0	29.7	19.1	27.5	48.4	31.3	15.1	51.9	27.8	48.6	35.4	64.4	33.2
201 to 500	24.2	13.3	18.9	22.3	21.6	24.7	19.4	40.3	19.8	15.7	20.3	23.1	17.8	22.4
501 to 700	9.1	9.3	13.5	21.3	15.7	11.8	11.9	42.9	14.8	4.3	14.9	6.2	4.4	15.5
701 or more	6.1	25.3	29.7	36.2	17.6	8.6	20.9	0.0	13.6	29.6	16.2	9.2	2.2	17.0
Total	100.0	99.9	99.9	100.0	100.0	100.0	99.9	100.0	100.1	100.0	100.0	100.1	99.9	100.0***
Percentage distribution of hours in all activities														
None	15.2	36.0	8.1	1.1	17.6	6.5	13.4	1.7	0.0	20.9	0.0	24.6	11.1	11.3
Up to 200	33.3	16.0	29.7	19.1	27.5	48.4	29.9	15.1	34.6	16.5	37.8	27.7	64.4	28.6
201 to 500	30.3	13.3	18.9	22.3	21.6	24.7	20.9	40.3	35.8	21.7	24.3	29.2	15.6	25.5
501 to 700	9.1	9.3	13.5	21.3	15.7	11.8	13.4	42.9	8.6	7.0	14.9	3.1	4.4	15.2
701 or more	12.1	25.3	29.7	36.2	17.6	8.6	22.4	0.0	21.0	33.9	23.0	15.4	4.4	19.5
Total	100.0	99.9	99.9	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	99.9	100.1***
Average length of stay (months)	5.5	4.4	4.5	10.1	5.4	7.1	6.5	5.2	5.8	8.1	10.7	5.5	6.3	6.8***

(continued)

TABLE 3.9 (continued)

	Concurrent								Sequential/ In-House		Sequential/ Brokered			
Activity Measure	Atlanta Job Corps	CET/ San Jose	Chicago Commons	Connelley (Pittsburgh)	East LA Skills Center	EGOS (Denver)	Phoenix Job Corps	SER/ Corpus Christi	El Centro (Dallas)	LA Job Corps	Allentown (Buffalo)	BSA (NYC)	CREC (Hartford)	Total
Percent still participating in month														
3	69.7	57.3	64.9	97.9	66.7	79.6	79.1	89.1	85.2	72.2	91.9	63.1	66.7	78.0***
6	36.4	40.0	45.9	76.6	51.0	57.0	50.7	60.5	44.4	48.7	78.4	38.5	40.0	53.6
9	21.2	21.3	8.1	64.9	27.5	34.4	32.8	0.0	24.7	38.3	55.4	26.2	28.9	30.6
12	12.1	8.0	0.0	24.5	7.8	21.5	19.4	0.0	6.2	26.1	41.9	15.4	22.2	16.4
15	6.1	1.3	0.0	17.0	0.0	7.5	6.0	0.0	0.0	23.5	29.7	10.8	11.1	9.6
18	3.0	0.0	0.0	11.7	0.0	2.2	1.5	0.0	0.0	16.5	10.8	3.1	4.4	4.8
19 or later	3.0	0.0	0.0	7.4	0.0	0.0	1.5	0.0	0.0	13.0	10.8	1.5	4.4	3.7
Number of experimentals	33	75	37	94	51	93	67	119	81	115	74	65	45	949

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. An F-statistic was used to test the hypothesis of equal column means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] The Phoenix Job Corps did not report hours spent by participants in life skills or avocational activities.

Further, the demonstration was not designed to address this type of question with the same rigor provided for comparisons of experimentals and controls. Applicants were randomly assigned to the experimental or control group, but there was no random assignment to various types of sites, and within each labor market there was usually only one site. This means that the power of the random assignment research design applies to differences between experimentals and controls (at a site or in the aggregate) and for differences among subgroups (as defined by pre-random assignment characteristics). Other types of comparisons, such as between types of sites, are inherently less reliable, and the strength of conclusions depends on the consistency of results across sites.[23]

Analysis presented in a previous report found that differences in experimentals' characteristics among the sites explained only a small part of the differences in average total hours.[24] The analysis also found that labor market conditions affected participation: Sites with better employment opportunities, other things being equal, had lower average participation hours.[25]

The strongest influence on participation in JOBSTART appeared to be program structure: whether a site was concurrent, sequential/in-house, or sequential/brokered. Participation rates by component, participation hours, and percentage of time in education or training all differed among the three types of sites, as detailed in Table 3.9 and summarized in Table 3.10.[26] Four conclusions about program structure can be drawn:

- **Average hours of participation varied by type of site.** Experimentals in sequential/in-house sites had the highest average participation hours, while those in sequential/brokered sites had by far the lowest because of very low average hours in training.

- **The mix of education, training, and other activities varied by type of site.** The concurrent sites, other than two Job Corps sites, did not offer the optional "other activities" and emphasized occupational training; as

---

[23] See Chapter 6 for a more detailed discussion of the difficulty of making cross-site comparisons.

[24] See Auspos et al., 1989.

[25] This could have been because those participating in JOBSTART found a job more easily and left the program after fewer hours. Alternatively, sites in labor markets with low unemployment may recruit youths who have more unmeasured barriers to employment, are harder to work with in a program, and end up with fewer hours of participation.

[26] As discussed earlier, eight sites provided concurrent basic education and occupational skills training ("concurrent" sites); two provided education followed by training ("sequential/in-house" sites); and three provided education and then referred participants to other agencies for training ("sequential/brokered" sites).

TABLE 3.10

PARTICIPATION RATES, HOURS OF PARTICIPATION, AND LENGTH OF STAY, BY PROGRAM STRUCTURE

Activity Measure	Concurrent	Sequential/ In-House	Sequential/ Brokered	Total
Percent participating in				
Education	84.9	86.7	88.0	85.9
Training	85.2	51.0	25.5	66.6***
Education and training	81.5	51.0	25.5	64.4***
Other activities	14.2	87.8	69.0	40.0***
Average hours in				
Education	103.7	151.7	178.4	128.1***
Training	291.3	286.6	77.7	248.9***
Education and training	395.0	438.3	256.1	377.0***
Other activities	10.0	95.1	60.0	37.3***
All activities	405.0	535.6	316.2	414.8***
Percentage distribution of hours in education and training				
None	11.4	13.3	12.0	11.9
Up to 200	26.9	37.8	47.8	33.2
201 to 500	24.8	17.3	20.7	22.4
501 to 700	19.9	8.7	9.2	15.5
701 or more	17.0	23.0	10.3	17.0
Total	100.0	100.1	100.0	100.0***
Percentage distribution of hours in all activities				
None	20.9	13.4	1.7	11.3
Up to 200	16.5	29.9	15.1	28.6
201 to 500	21.7	20.9	40.3	25.5
501 to 700	7.0	13.4	42.9	15.2
701 or more	33.9	22.4	0.0	19.5
Total	100.0	100.0	100.0	100.1***
Average length of stay (months)	6.4	7.1	7.8	6.8***
Percent still participating in month				
3	78.9	77.6	75.5	78.0***
6	55.5	46.9	54.9	53.6
9	27.2	32.7	38.6	30.6
12	12.3	17.9	27.7	16.4
15	5.3	13.8	18.5	9.6
18	2.6	9.7	6.5	4.8
19 or later	1.6	7.7	6.0	3.7
Number of experimentals	569	196	184	949

(continued)

TABLE 3.10 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Distributions may not total 100.0 percent because of rounding.

A Pearson chi-square statistic was used to test the hypothesis of equal distributions. An F-statistic was used to test the hypothesis of equal column means. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

a result, average training hours for experimentals amounted to 72 percent of average total hours.[27] The sequential/brokered sites emphasized education and other non-training services, all of which were provided in-house. They had the highest average hours in education; training hours were only about 25 percent of average total hours. The sequential/in-house sites had the highest total average hours, hours in education and other non-training activities approximating those of the sequential/brokered sites, and hours in training like those of the concurrent sites.

- **Sequential/brokered sites had difficulty moving participants from education to training.** Only 26 percent of participants at sequential/brokered sites made the transition to occupational training, although those who made the transition did receive substantial training. This low rate of participation in training occurred because of the difficulty of linking participants with other organizations, in part because of the nature of typical JTPA Title IIA contracts. Possibly, it also arose because participants in these sites (which were primarily basic education organizations) were more interested in receiving a GED than occupational training.

- **These relationships do not appear to have been the result of measured differences in participant characteristics or local employment opportunities.** Even after adjustments for measured differences in participant characteristics and local employment opportunities, these patterns of participation among sites with different program structures still appear.[28]

While these three categories of sites do clarify patterns of participation, the sites within each category were clearly not identical. Among the concurrent sites, EGOS in Denver stood out with especially low hours – possibly because of its very large size, which could have left the JOBSTART youths feeling isolated and disconnected from the program. CREC in Hartford, among the sequential/brokered sites, had very low hours because it scheduled only three hours of education per day and very few experimentals participated in training. Furthermore, CREC offered limited support services and moved several times during the demonstration, which disrupted program operations. The high total hours for sequential/in-house sites were primarily owing to the Los Angeles Job Corps, which had the highest average hours among all sites. El Centro in Dallas, the other site in this category, ranked slightly below the average for all sites in total hours.

---

[27] The percentage of average total hours is calculated by dividing average training hours by average total hours. Percentages that follow are similarly calculated.

[28] When dummy variables for type of site were added as independent variables to a regression equation with individual demographic characteristics and a measure of local employment opportunities, the relationships still held.

IV. Summary of the Features of the Sites

The diversity of the sites within the general framework of the JOBSTART guidelines has been a theme of this chapter. But before moving on to consider the impacts of these programs, it is useful to summarize in one place the key features of each local JOBSTART program. Table 3.11 groups the sites by program structure (concurrent, sequential/in-house, sequential/brokered) and uses three types of measures to summarize JOBSTART implementation. First, it describes participation in JOBSTART for experimentals by presenting average total hours; average length of stay in the program; average hours per month in the program; and average hours of education and training. (Note that each of these measures includes the 11 percent of experimentals who did not participate in the program.) All of these items have been included in previous tables except for average hours per month, which is a measure of the extent to which program services were concentrated or spread out over time.

Table 3.11 also includes more subjective ratings of implementation, including a rating of the level of initial screening done by each site at intake. Initial screening was greatest at the three Job Corps Centers (which had special entrance criteria) and Chicago Commons (which had special requirements for entering its vocational training program). The table also includes ratings of job placement and support services (drawn from Table 3.4) and an overall assessment of JOBSTART implementation.[29]

Finally, the cost per experimental in each site's research sample, listed in the right column of this table, adds to the description of program implementation already presented in this chapter.[30] Most programs tend to fall in the range of $4,500 to $6,500, but several fall

---

[29] Chapter 6, which discusses differences in impacts among the sites, contains a further summary table of each site's program.

[30] See Appendix E for the details of these cost data, which include items not normally part of program budgets, such as the value of donated goods and services. These cost figures are not the ones that would be produced as part of a benefit-cost analysis that compares the impact of the program to the incremental cost of the services received by experimentals above the level of services received by controls. As is discussed in Chapter 4, the controls received substantial non-JOBSTART services. Thus the impact figures estimate the effect of the services received by experimentals above this base of services. Costs used to calculate a benefit-cost ratio would measure the resource cost of the *extra* services received by experimentals. The cost figures reported here are gross program costs, before subtracting the cost of services received by controls.

TABLE 3.11

SUMMARY OF JOBSTART IMPLEMENTATION, BY SITE

Site	Total Hours	Length of Stay (Months)	Average Hours per Month	Hours of		Level of Initial Screening	Rating of		Overall Rating of Implementation	JOBSTART Operating Costs per Experimental ($)[a]
				Education	Training		Job Placement	Support Services		
Concurrent	405	6.4	60	104	291	---	---	---	---	---
Atlanta Job Corps	296	5.5	50	95	150	High	Low	High	Low	4,100[b]
CET/San Jose[c]	366	4.4	74	29[d]	337	Low	High	Medium	High	2,000
Chicago Commons[c]	445	4.5	83	72[d]	373	High	High	Medium	High	6,400
Connelley (Pittsburgh)	579	10.1	54	105	473	Medium	Medium	High	High	5,200
East LA Skills Center	345	5.4	55	73	272	Medium	Low	Medium	Medium	4,900
EGOS (Denver)	274	7.1	33	126	148	Low	Low	Medium	Low	2,000[b]
Phoenix Job Corps	441	6.5	60	163	218	High	High	High	High	4,700[b]
SER/Corpus Christi	418	5.2	76	124	294	Medium	Low	High	Medium	2,100
Sequential/in-house	536	7.1	66	152	287	---	---	---	---	---
El Centro (Dallas)	401	5.8	60	142	179	Medium	Medium	Medium	High	5,100
LA Job Corps	631	8.1	71	159	362	High	Medium	High	High	5,700[b]
Sequential/brokered	316	7.8	37	178	78	---	---	---	---	---
Allentown (Buffalo)	440	10.7	39	244	113	Medium	Low	High	Medium	5,900
BSA (NYC)	280	5.5	45	145	66	Low	Low	Medium	Low	7,500
CREC (Hartford)	166	6.3	23	119	36	Low	Low	Medium	Low	5,200
All sites	415	6.8	57	128	249	---	---	---	---	---

SOURCES: MDRC calculations from JOBSTART enrollment form, MIS, and survey data (participation figures); MDRC operations staff (implementation ratings); Appendix E (costs).

NOTES: [a] These cost estimates are preliminary and therefore rounded to the nearest $100. All costs are in 1986 dollars.

[b] The estimates do not include the cost of providing on-site medical and dental services. The value of these services per JOBSTART experimental was approximately $400 in the Atlanta site, $400 in Phoenix, $600 in Los Angeles, and $24 in Denver.

[c] In this site, education and training were more integrated than in other sites, and staff strongly emphasized training over passing the GED examination.

[d] In this site, some education hours are included in the training component hours.

well outside this range. The variation in program costs is substantial, reflecting several factors:

- **Enrollment levels.** Programs serving a higher volume of participants relative to their institutional capacity can spread the fixed costs of operation over many people, thereby lessening the average cost. EGOS in Denver, a large adult vocational school with more than 15,000 students, thus had very low costs per experimental. In contrast, BSA in New York City had difficulty enrolling enough students to fill all of its available slots and as a result had very high costs per experimental.

- **Intensity of the planned components.** More ambitious training or support services cost more per month of operation. This was the case at Chicago Commons, where the training options were often quite intensive.

- **Average length of participation.** Since costs per experimental depend on the cost per unit of service and the number of units used, sites with longer participation, such as the Los Angeles Job Corps, have higher costs.

- **Staff salary levels.** Personnel costs are the most important part of costs, so an agency's salary structure and level are major determinants of cost. The low average cost of SER/Corpus Christi is partly explained by that site's relatively low personnel expenses.

The following two chapters present program impacts, for the sample as a whole and for key subgroups defined by individual characteristics. The final chapter of the report returns to the issue of site variation, in reviewing the pattern of program impacts among the 13 sites.

CHAPTER 4

RECEIPT OF EDUCATION AND SKILLS TRAINING AND JOBSTART'S IMPACTS ON EDUCATIONAL ATTAINMENT

The goals of the JOBSTART program included increasing participation in education and training activities by a group who otherwise would be little served, and thereby enhancing their educational attainment, employability, and long-term earnings. Chapter 3 described the experimental group's participation in JOBSTART activities.[1] This chapter takes the story a step farther by comparing the participation of these experimentals in JOBSTART (and other programs) with the activities of those in the control group, to determine whether the offer of JOBSTART services actually led to an increase in participation over what would have occurred otherwise.[2] As will be shown, the youths in the experimental group did participate in more education and training than did those in the control group, but the control group was not unserved. Chapters 5 and 6 examine the extent to which this increase in services has so far led to improvements in employment, earnings, welfare receipt, and other longer-term outcomes.[3]

This and succeeding chapters rest on the random assignment research design, described briefly in earlier chapters, to estimate the difference the JOBSTART program made in the lives of those young people given access to it. To evaluate these differences (often called program "impacts"), it is necessary to answer two basic questions. First, on average, what happened to those who were offered the program – in this context, the "experimental" group? Second, on average, what would have happened to them had they not been offered the program, here represented by the experience of the "control" group?[4] The average effect, or "impact," of a program is the difference between the two groups in the many outcomes of interest.

---

[1] This was based on attendance information reported by the JOBSTART program operators for those in the experimental group.

[2] In addition to using the JOBSTART attendance data from the program MIS, this chapter relies on survey data for both experimentals and controls gathered 12 and 24 months after random assignment. The survey data include participation in other programs as well as in JOBSTART.

[3] Chapter 5 focuses on the story for the full impact sample and for subgroups defined by individual characteristics such as gender, while Chapter 6 treats cross-site differences in impacts.

[4] Since, as shown in Chapter 2, assignment to JOBSTART was random, there were no systematic differences between experimentals and controls at enrollment, and outcomes for controls could be used to measure what would have happened to experimentals without the program.

This chapter addresses several key evaluation questions:

- Did the planned differential between experimentals and controls in the receipt of education and occupational skills training materialize? Was this differential maintained during the second year of follow-up, or had controls begun to catch up to experimentals?
- How did service receipt and the differential in service receipt compare for important subgroups such as men, young mothers, and other women?
- Did JOBSTART produce gains in educational attainment, as measured by receipt of high school diplomas and passing of the GED examination, during the first two years after random assignment? Were controls catching up to the experimentals in educational attainment during the second year?
- How did the educational attainment differential compare for important subgroups such as men and women?

To summarize the basic findings in this chapter, access to JOBSTART did substantially increase the experimentals' participation in education and training activities, raising their rates and average hours of participation well above those of controls, almost half of whom were also active in these types of programs. This greater service receipt among experimentals occurred for all important subgroups in the sample; it led to substantially higher rates of receipt of a GED or high school diploma among experimentals than among controls for the full impact sample and for most subgroups.

I. Receipt of Education and Training by Experimentals and Controls

The JOBSTART control group was used as a benchmark for measuring program impacts, but if most controls received services similar to those received by experimentals, the benchmark would have been useless, and it would be very difficult to evaluate JOBSTART.[5] Because JOBSTART targeted disadvantaged dropouts with poor reading skills (a group thought hard to serve within the JTPA system), demonstration planners anticipated that JOBSTART controls would not be served to any great extent.

---

[5] The service receipt differences reported here are calculated by comparing the experiences of experimentals and controls as is done for analysis of the impact of JOBSTART on outcomes such as educational attainment and employment. These service receipt differences are not normally thought of as program impacts because they are an intermediate step in reaching the final program goals. This section uses the terminology of program impacts at times when it simplifies the presentation of the results. This terminology is useful here because the chapter discusses differences among subgroups in the size of these experimental-control differences; the use of the term impact for the experimental-control difference simplifies sentences.

However, JOBSTART controls were expected to receive some services. Even though many performance-driven programs seem to screen out people with low reading levels, JOBSTART recruits (including those who ended up in the control group) were more determined to receive help than was the average school dropout. As a result, at some point within the first 24 months following random assignment, 44 percent of controls found remedial or occupational instruction elsewhere. The program impacts on educational attainment, employment, and earnings presented here, therefore, are the incremental impacts of JOBSTART over the mix of services available throughout the community to a group of poor readers, on their own initiative, without special referral from JOBSTART site operators.

A. In-Program and Post-Program Outcomes

All the events tracked by the JOBSTART program attendance reporting system and the follow-up survey (including program participation, GED receipt, employment, and other important outcomes) were reckoned from the date of random assignment, not the date of termination from the program.[6] There was a great deal of variation in lengths of stay in JOBSTART. However, an approximate dividing line between predominantly in-program and predominantly post-program periods is no earlier than about the end of the twelfth month after random assignment. About 84 percent of experimentals had stopped participating in JOBSTART by then. (See Table 3.5.)

Table 4.1 shows that, over the two-year period as a whole, 92.7 percent of experimentals and 44.2 percent of controls received some education or training, for an impact of 48.4 percentage points. This impact was statistically significant, that is, too big to have arisen entirely by chance. As shown in Figure 4.1, the proportion of experimentals in programs, mainly JOBSTART, was highest during the first three months and fell rather steadily over time, to 11.2 percent during month 24. The proportion of controls in programs was much smaller at the beginning of the follow-up period, peaked at about 15 percent during months

---

[6] This is a different approach from that used in the Job Corps study, in which follow-up began at termination from the program. As will become clear in Chapter 5, starting follow-up with entry into the program allows a careful analysis of the forgone earnings caused by participation in the program. But this difference in approach complicates the comparison of JOBSTART and Job Corps impact findings.

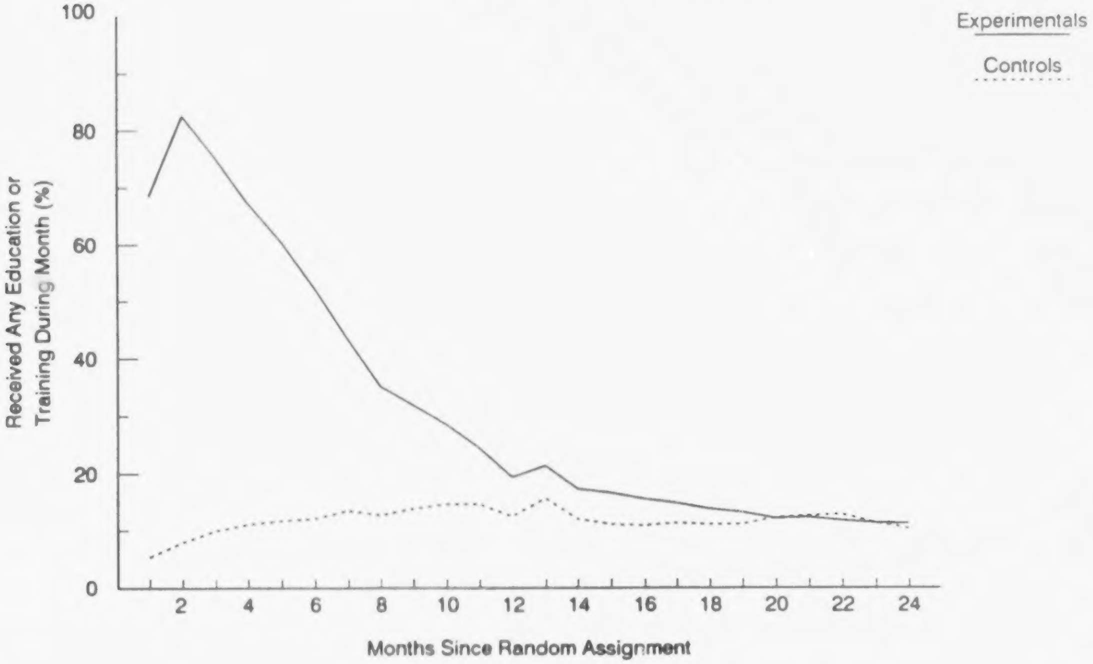
TABLE 4.1

IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 24

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Ever received any education or training, months 1-24	92.7%	44.2%	48.4***	0.000
Ever received any education or training, months 1-12	90.7	29.0	61.6***	0.000
Ever received any education or training, months 13-24	34.3	30.7	3.6*	0.089
Sample size	949	890		

FIGURE 4.1

MONTHLY RECEIPT OF EDUCATION OR TRAINING, BY RESEARCH STATUS



(continued)

TABLE AND FIGURE 4.1 (continued)

SOURCES FOR TABLE AND FIGURE 4.1: MDRC calculations from JOBSTART enrollment form, MIS, and survey data (Table 4.1); Appendix Table F.1 (Figure 4.1).

NOTES FOR TABLE AND FIGURE 4.1: Calculations for this table and figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

10 through 13, and then soon reached a plateau at about 10 to 12 percent, about even with the level to which experimentals had fallen.

Breaking the follow-up period into a predominantly in-program period (months one through 12) and a predominantly post-program period (months 13 through 24) reveals a more dramatic pattern of experimental and control differences. Table 4.1 shows that during the first 12 months of follow-up, 90.7 percent of experimentals participated in education or training compared to 29 percent of controls, for an impact of 61.6 percentage points. During the second 12 months, 34.3 percent of experimentals and 30.7 percent of controls were in education or training, for an impact of 3.6 percentage points. Thus, there is no evidence that controls are catching up with experimentals in education and training. On the contrary, experimentals had not fallen below controls in education and training even in the second year of follow-up; both groups had stabilized at about the same rate of service receipt by the end of the second year.

Hours of education or training followed a very similar pattern. Table 4.2 shows that over the two-year period, experimentals received an average of 619 hours, while controls received an average of 250 hours, for an impact of 369 hours. It is important to remember that these average figures for the two groups *include* those who did not participate and, therefore, had zero hours. As Figure 4.2 shows, average experimental hours peaked at about 70 hours per month during month two and then fell steadily, while control hours, always much lower, peaked during month nine and then stabilized at about 10 hours per month.

Among those 92.7 percent of experimentals and 44.2 percent of controls who received any services during the two-year period – that is, excluding those who received no services – experimentals averaged 668 hours, and controls averaged 566 hours.[7] Because so many fewer controls received services, and those who did find services on their own were probably quite motivated, this difference in duration *for those who received services* may understate the true advantage in intensity for served experimentals. The served experimentals who were directly comparable in motivation to the served controls might well have been those who received many more than the average hours for all served experimentals.

Thus, the planned service differential between experimentals and controls materialized. Not only did experimentals receive education and training at vastly higher rates than did

---

[7] This is not shown in the table or figures.

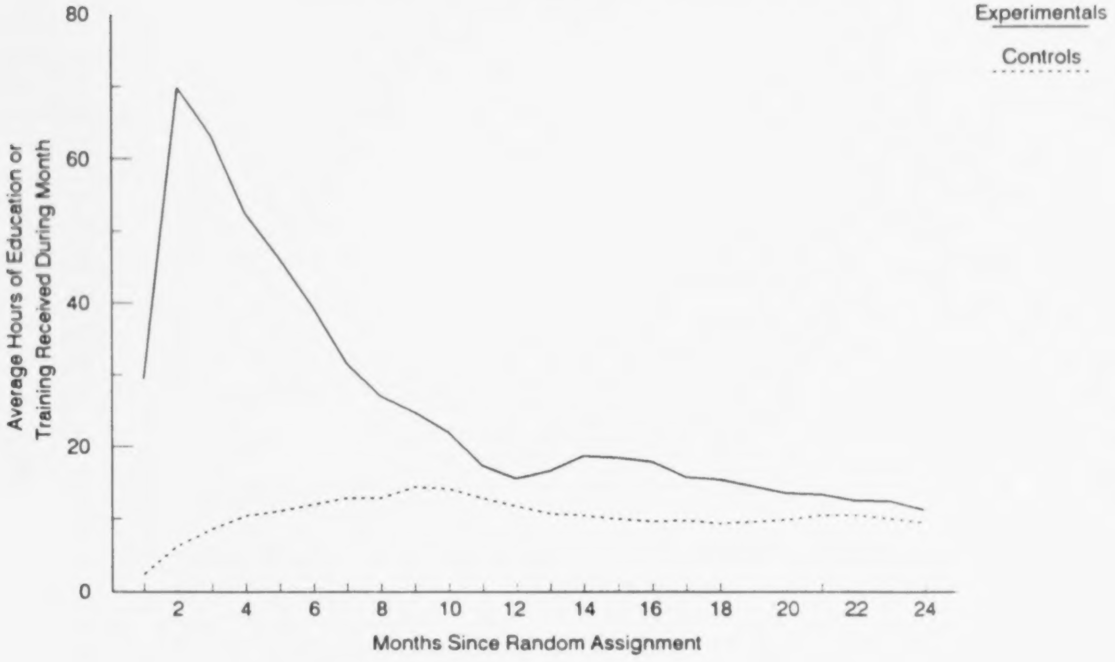
TABLE 4.2

IMPACTS ON TOTAL HOURS OF EDUCATION OR TRAINING RECEIVED THROUGH MONTH 24

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Total hours of education or training received, months 1-24	619.16	250.14	369.02***	0.000
Total hours of education or training received, months 1-12	439.02	130.06	308.96***	0.000
Total hours of education or training received, months 13-24	180.14	120.08	60.06***	0.001
Sample size	949	890		

FIGURE 4.2

MONTHLY AVERAGE HOURS OF EDUCATION OR TRAINING RECEIVED, BY RESEARCH STATUS



(continued)

TABLE AND FIGURE 4.2 (continued)

SOURCES FOR TABLE AND FIGURE 4.2: MDRC calculations from JOBSTART enrollment form, MIS, and survey data (Table 4.2); Appendix Table F.2 (Figure 4.2).

NOTES FOR TABLE AND FIGURE 4.2: Calculations for this table and figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

For experimentals, "hours of education or training" include JOBSTART hours from MIS data and non-JOBSTART hours from survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

controls throughout much of the 24-month follow-up period, but they also on average received many more hours. However, as noted earlier, the control group did receive some other services in the community. Thus, the findings presented in Chapters 4 through 6 represent the incremental impacts of JOBSTART above the existing level of service.

B. Differences in Receipt of Education and Training by Men and Women

Table 4.3 results from splitting the sample into three groups (men, women living with their own children, and other women) and repeating the calculations made for the full sample in Tables 4.1 and 4.2. In general, Table 4.3 shows that among the experimental group, men, women living with their own children, and other women tended to be active in all types of education and training activities in approximately similar amounts in the first year.[8] However, there were clear differences in participation among the control groups for the three subgroups: Men in the control group participated in education and training less than did women living with their own children, who in turn participated less than other women. The larger *differences* between experimentals and controls for men among those who ever participated and in average total hours, therefore, result largely from the lower level of activity among men in the control group.

To summarize the service receipt differences shown in Table 4.3: During the two-year follow-up period, 92.8 percent of men in the experimental group and 36.6 percent in the control group were active in education or training, for a difference of 56.1 percentage points. This resulted in a difference of 424 average total hours between men in the experimental and control groups. For women living with their own children, 45.7 percentage points more experimentals were active during the two-year follow-up period, and experimentals averaged 344 more hours than did controls. For other women, the comparable figures were 38.4 percentage points and 303 hours.

---

[8] This finding is based on analysis of the proportion of experimentals in each group who "ever participated" in an activity during the period in question. The finding holds during the first year of follow-up and over the entire 24-month follow-up period. In the second year of follow-up (and over the entire two-year follow-up period), more "other women" participated in an activity (registering the highest average total hours in the second year). Women living with their own children had the next highest average total hours, and men had the lowest. These differences were largely attributable to differences in participation in non-JOBSTART activities. As reported in Chapter 3, participation in JOBSTART activities was similar among the three groups.

TABLE 4.3

IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Ever received any education or training (%)				
Months 1-24	92.8	36.6	56.1***	0.000
Months 1-12	90.5	24.1	66.4***	0.000
Months 13-24	29.5	23.3	6.2**	0.038
Total hours of education or training received				
Months 1-24	604.57	180.12	424.45***	0.000
Months 1-12	444.93	94.95	349.98***	0.000
Months 13-24	159.64	85.16	74.47***	0.002
Sample size	438	433		
Women living with own child(ren)				
Ever received any education or training (%)				
Months 1-24	93.6	47.9	45.7***	0.000
Months 1-12	91.1	29.2	61.9***	0.000
Months 13-24	38.4	35.5	2.9	0.522
Total hours of education or training received				
Months 1-24	599.32	255.57	343.75***	0.000
Months 1-12	411.36	126.06	285.30***	0.000
Months 13-24	187.96	129.52	58.45*	0.088
Sample size	250	234		
Women not living with own child(ren), including those who did not have any				
Ever received any education or training (%)				
Months 1-24	92.5	54.1	38.4***	0.000
Months 1-12	90.5	38.4	52.1***	0.000
Months 13-24	39.9	38.5	1.4	0.758
Total hours of education or training received				
Months 1-24	672.35	369.05	303.30***	0.000
Months 1-12	456.32	201.57	254.75***	0.000
Months 13-24	216.03	167.48	48.55	0.202
Sample size	261	223		

(continued)

TABLE 4.3 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities. For experimentals, "hours of education or training" include JOBSTART hours from MIS data and non-JOBSTART hours from survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

C. Differences in Receipt of Education and Training by People with Selected Characteristics

The primary goal of the evaluation is to estimate the difference that access to the JOBSTART program makes for its target population (in other words, the program's impacts). While the size of the JOBSTART sample is large enough for finding policy-relevant overall impacts, it provides considerably less statistical power for estimating subgroup impacts and differences in impacts among subgroups. Keeping this limitation in mind, this section presents an analysis of the difference in service receipt among experimentals and controls in various subgroups of youths and compares the size of this difference for the selected subgroups.

To summarize the findings of this section: The service receipt differences among experimentals and controls observed for the full sample were present and large for virtually all important subgroups. Observed differences in service receipt impacts among subgroups primarily reflected variation in the level of service receipt of controls.

Table 4.3 presents impacts calculated using the same methods as were used for the full sample: Impacts for the three subgroups were calculated by comparing the experiences of the experimentals in each group to those of the controls in the group. Table 4.4 uses a different method to calculate within-subgroup impacts and between-subgroups impact differences for the most important measure of program activity: receipt of education or training during the two years of follow-up available so far. It presents impacts for each subgroup, controlling for differences other than the characteristic used to define the subgroup.[9]

For example, the first three rows of Table 4.4 present impacts for women and men using statistical techniques to control for gender differences, such as in employment experience, educational levels, ethnicity, and parental status. It thus shows a comparison of the impacts by gender with other characteristics held constant. The first row of Table 4.4, in the column labeled "subgroup impact difference," shows that the impact for women was 13.4 percentage points below the impact for men, and this difference in service receipt impacts was statistically

---

[9] The impact estimates in this table were produced by conducting a two-way analysis of covariance, controlling for differences in pre-random assignment characteristics other than the characteristic used to define the subgroup. See Ostle, 1975, p. 461. The adjustments were done using a linear regression model. Characteristics that affect outcomes and impacts with a nonlinear relationship are not controlled for with this procedure. And no such procedure can control for unmeasured characteristics that affect outcomes and impacts. The adjusted outcomes for men, women living with their own children, and other women presented in Table 4.3 are very similar to the unadjusted outcomes in Table 4.2, but the adjustments do make more of a difference for many of the other subgroups listed in the table.

TABLE 4.4

IMPACTS ON RECEIPT OF EDUCATION OR TRAINING THROUGH MONTH 24, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

		Ever Received Any Education or Training, Months 1-24					
Characteristic and Subgroups	Sample Size	Experimentals	Controls	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Gender						-13.4***	0.000
Women	968	93.5%	51.4%	42.1***	0.000	---	---
Men	871	91.9	36.4	55.5***	0.000	---	---
Ethnicity						---	0.260
White, non-Hispanic	155	93.8	38.9	54.9***	0.000	---	---
Black, non-Hispanic	840	91.9	47.1	44.7***	0.000	---	---
Hispanic	783	92.7	42.1	50.6***	0.000	---	---
Other	61	101.8	47.0	54.8***	0.000	---	---
Ethnicity, by gender						---***	0.003
Women							
White, non-Hispanic	84	92.4	42.8	49.6***	0.000	---	---
Black, non-Hispanic	445	94.5	54.2	40.3***	0.000	---	---
Hispanic	413	92.0	50.9	41.1***	0.000	---	---
Other	26	106.6	45.0	61.5***	0.000	---	---
Men							
White, non-Hispanic	71	96.1	34.1	61.9***	0.000	---	---
Black, non-Hispanic	395	88.9	39.2	49.6***	0.000	---	---
Hispanic	370	93.7	32.6	61.1***	0.000	---	---
Other	35	97.0	46.7	50.4***	0.000	---	---
Parental status						---***	0.001
Women living with own child(ren)							
No	484	93.7	55.8	38.0***	0.000	---	---
Yes	484	90.5	44.3	46.1***	0.000	---	---
Men who have own child(ren)							
No	765	94.1	38.3	55.7***	0.000	---	---
Yes	106	89.4	35.9	53.5***	0.000	---	---

(continued)

TABLE 4.4 (continued)

		Ever Received Any Education or Training, Months 1-24					
Characteristic and Subgroups	Sample Size	Experimentals	Controls	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Employed within past year						-6.0*	0.097
No	870	92.4%	47.2%	45.3***	0.000	---	---
Yes	969	92.9	41.6	51.3***	0.000	---	---
Prior employment, by gender						---***	0.001
Women employed within past year							
No	547	96.0	52.9	43.1***	0.000	---	---
Yes	421	90.9	50.2	40.8***	0.000	---	---
Men employed within past year							
No	323	88.5	39.6	49.0***	0.000	---	---
Yes	548	93.4	34.1	59.3***	0.000	---	---
Left school in grade 11 or 12						0.9	0.811
No	1,078	93.6	44.8	48.8***	0.000	---	---
Yes	761	91.4	43.5	47.9***	0.000	---	---
Received occupational training within past year						-4.9	0.302
No	1,529	92.7	45.2	47.5***	0.000	---	---
Yes	310	92.2	39.8	52.5***	0.000	---	---
Age						4.9	0.230
16-19	1,359	93.4	43.7	49.7***	0.000	---	---
20 or 21	480	90.4	45.7	44.8***	0.000	---	---
Age, by gender						---***	0.002
Women							
16-19	710	94.0	50.9	43.0***	0.000	---	---
20 or 21	258	92.5	52.9	39.6***	0.000	---	---
Men							
16-19	649	93.1	36.1	57.0***	0.000	---	---
20 or 21	222	88.2	37.1	51.1***	0.000	---	---

(continued)

TABLE 4.4 (continued)

Characteristic and Subgroups	Sample Size	Ever Received Any Education or Training, Months 1-24 Experimentals	Controls	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Marital status						1.7	0.779
Ever married	174	97.4%	47.4%	50.0***	0.000	---	---
Never married	1,665	92.2	43.9	48.3***	0.000	---	---
Living in own household or with boy/girlfriend						7.0	0.134
No	1,500	93.3	43.6	49.7***	0.000	---	---
Yes	339	89.7	46.9	42.7***	0.000	---	---
Own AFDC case or receiving General Assistance						9.8**	0.016
No	1,344	93.3	42.3	51.1***	0.000	---	---
Yes	495	90.7	49.4	41.3***	0.000	---	---
Own AFDC case						10.8**	0.014
No	1,446	92.7	42.0	50.7***	0.000	---	---
Yes	393	92.4	52.4	39.9***	0.000	---	---
Receiving Food Stamps						10.9***	0.003
No	1,143	95.5	42.9	52.5***	0.000	---	---
Yes	696	88.1	46.4	41.7***	0.000	---	---
Arrested since age 16						-6.6	0.194
No	1,567	92.5	45.1	47.4***	0.000	---	---
Yes	272	93.3	39.2	54.1***	0.000	---	---
Lived with both parents at age 14						-3.5	0.352
No	1,198	91.8	44.6	47.2***	0.000	---	---
Yes	641	94.3	43.6	50.7***	0.000	---	---

(continued)

TABLE 4.4 (continued)

	Ever Received Any Education or Training, Months 1-24						
Characteristic and Subgroups	Sample Size	Experimentals	Controls	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Reason for leaving regular high school						---**	0.025
School-related	886	92.9%	41.1%	51.8***	0.000	---	---
Job-related	182	93.6	37.6	56.0***	0.000	---	---
Other	771	92.3	49.4	42.9***	0.000	---	---

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.

significant. The "subgroup impact" column shows that this difference in impacts was calculated as 55.5 percentage points (the impact for men, shown in the second row) minus 42.1 percentage points (the impact for women, shown in the third row).

Other important subgroup comparisons of impacts on service receipt include:

- **Age.** Very different patterns of labor market behavior are exhibited at each age in the general youth population. Labor force participation, employment, and earnings increase dramatically from age 16 to the early twenties. Thus, holding everything else constant, youth over age 20 would pay higher opportunity costs for program attendance than would the younger people. The impacts provide some support for this generalization, with teenage sample members having a slightly higher impact on service receipt (49.7 percentage points) than older youths in the sample (44.8 percentage points).

- **Prior employment.** For those with a more extensive work history, as evidenced by employment in the year before random assignment, the opportunity costs of participating in a program may be greater. Experimentals with and without prior employment had high rates of participation in education and training, though the impacts on service receipt for those with prior employment were higher because of the lower rate of participation in programs by controls.

- **Highest grade attended.** While all JOBSTART enrollees were high school dropouts, some left school before completing the tenth grade, while others dropped out during their junior or senior years. Despite the differences in past success in school, the levels of participation and impacts on service receipt were nearly identical for the two groups of youths. Apparently, JOBSTART sites found ways to engage the lower-attainment youths in an education and training program.

- **Welfare receipt.**[10] Those who receive AFDC, General Assistance, or Home Relief may tend to get higher levels of support services such as child care, and sometimes may be mandated to participate in some program in order to maintain eligibility for their cash benefits. Impacts for those not receiving welfare at random assignment were higher than for welfare recipients because control group welfare recipients were more likely to participate in an education or training program.

D. Receipt of Education and Training Separately

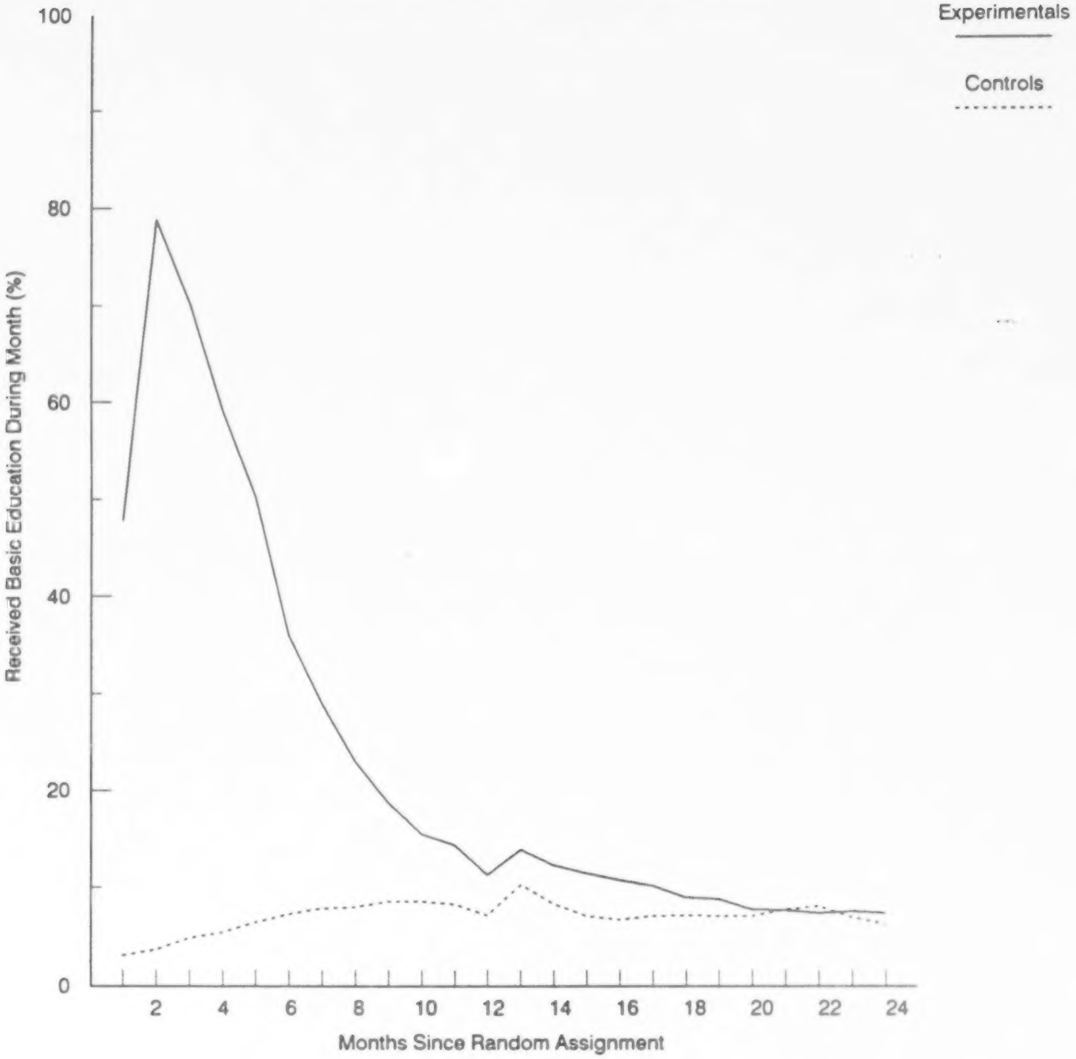
Figure 4.3 reports separately on the monthly receipt of education, showing a large

---

[10] Since only about 13 percent of the men in the sample received AFDC or General Assistance at random assignment, the subgroup receiving welfare was primarily made up of women.

FIGURE 4.3

MONTHLY RECEIPT OF BASIC EDUCATION, BY RESEARCH STATUS



SOURCE: Appendix Table F.3.

NOTES: Calculations for this figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Basic education" includes JOBSTART and non-JOBSTART education activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

difference between experimentals and controls in their participation in education throughout much of the two-year follow-up period. During the first year, 88 percent of experimentals participated in education compared to 17 percent of controls, for a difference in education participation of 71 percentage points. In the second year, the participation of experimentals dropped sharply as they left the JOBSTART program, while that of controls remained approximately what it was during the first year, causing the service receipt difference to decline to only 4 percentage points. Analysis of subgroup impacts again indicates a somewhat stronger service differential for men than for women, because women controls tended to get more education services on their own than did men controls.

Figure 4.4 shows separately JOBSTART's impact on the receipt of training. JOBSTART achieved less of an experimental-control differential for training alone than for training and education together.[11] The smaller impact for training than for education resulted from the failure of some JOBSTART sites to achieve transitions into training from education, as described in Chapter 3. Similar patterns of impacts on receipt of training appeared for men and women, but again the impacts tended to be larger for men.

II. Impacts on Educational Attainment

As indicated in the 1989 interim report, *Implementing JOBSTART*, the impacts of JOBSTART on educational attainment during the first year of follow-up were quite similar to those of the program that inspired it, the residential Job Corps. An evaluation of the Job Corps found that 24 percent of Corpsmembers, but only 5 percent of the comparison group, had high school diplomas or GEDs six months after termination from the program (the period roughly equivalent to a year of post-random assignment follow-up).[12] This report carries the story forward to the end of the two-year follow-up period.

JOBSTART impacts on educational attainment are presented in Table 4.5 for the full sample. Table 4.6 presents separate results for men, women living with their own children, and other women. The severe and intractable problems in reading and mathematics for the young adults in JOBSTART are reflected in the low rates of completing high school or passing the

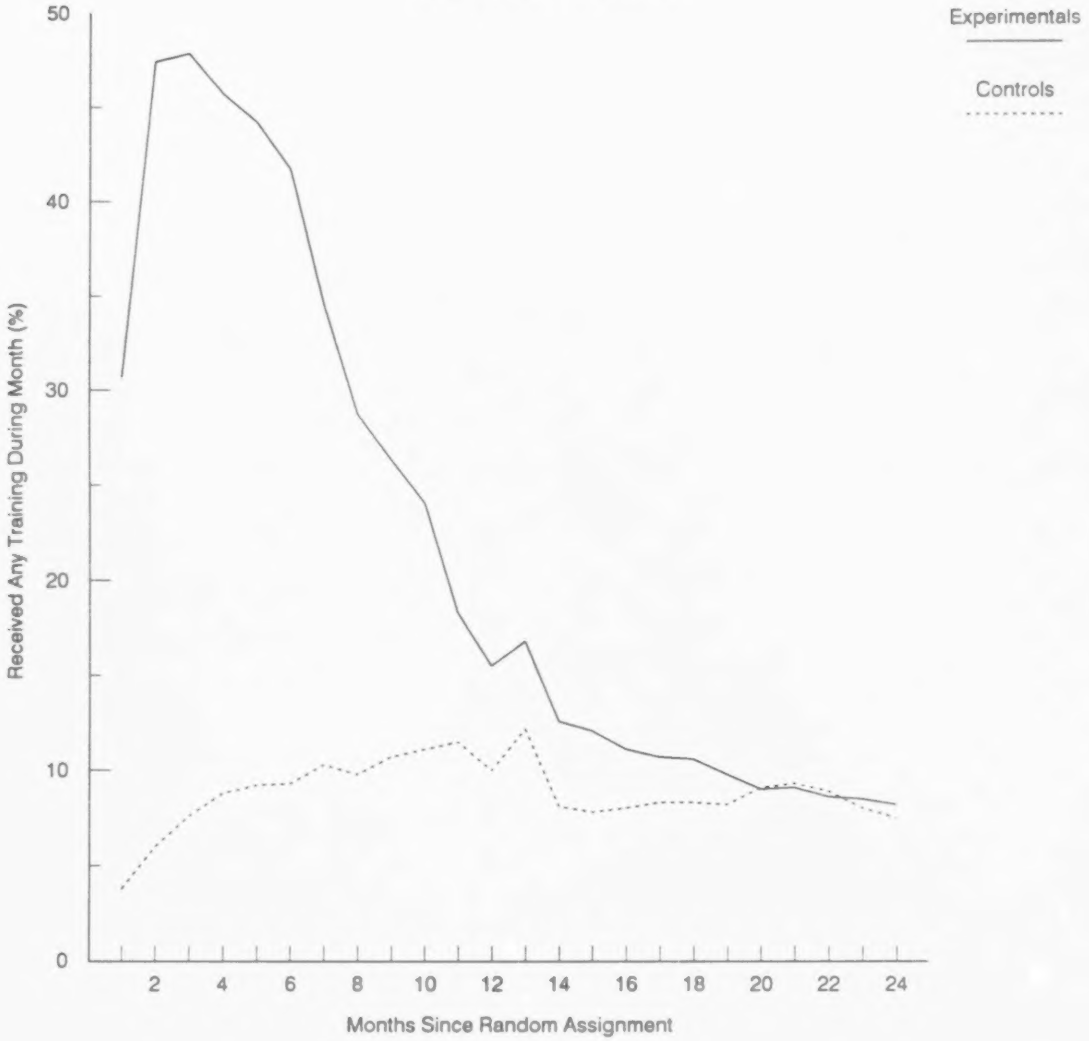
---

[11] The two-year impact on receipt of training was 41.6 percentage points for the full sample, 36.0 percentage points among women, and 49.0 percentage points among men. A comparison with the figures for education or training in Table 4.1 supports this point in the text.

[12] Mallar et al., 1982. See also Betsey et al., 1985, p. 112.

FIGURE 4.4

MONTHLY RECEIPT OF TRAINING, BY RESEARCH STATUS



SOURCE: Appendix Table F.4.

NOTES: Calculations for this figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Training" includes JOBSTART and non-JOBSTART occupational skills training activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

TABLE 4.5

IMPACTS ON EDUCATIONAL ATTAINMENT THROUGH MONTH 24

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Received GED by end of month 24	30.7%	11.9%	18.9***	0.000
Received GED or high school diploma by end of month 24	33.2	16.4	16.7***	0.000
Received trade certificate or license by end of month 24	19.2	8.9	10.3***	0.000
Received associate's or 2-year college degree by end of month 24	0.2	0.0	0.2	0.146
Received bachelor's or 4-year college degree by end of month 24	0.0	0.0	0.0	1.000
Sample size	949	890		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 4.6

IMPACTS ON EDUCATIONAL ATTAINMENT THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Received GED by end of month 24 (%)	28.8	12.0	16.8***	0.000
Received GED or high school diploma by end of month 24 (%)	32.1	16.9	15.2***	0.000
Received trade certificate or license by end of month 24 (%)	22.1	9.1	12.9***	0.000
Received associate's or 2-year college degree by end of month 24 (%)	0.0	0.0	0.0	1.000
Received bachelor's or 4-year college degree by end of month 24 (%)	0.0	0.0	0.0	1.000
Sample size	438	433		
Women living with own child(ren)				
Received GED by end of month 24 (%)	33.6	10.6	23.0***	0.000
Received GED or high school diploma by end of month 24 (%)	35.5	14.2	21.3***	0.000
Received trade certificate or license by end of month 24 (%)	16.7	12.1	4.6	0.148
Received associate's or 2-year college degree by end of month 24 (%)	0.4	0.0	0.4	0.297
Received bachelor's or 4-year college degree by end of month 24 (%)	0.0	0.0	0.0	1.000
Sample size	250	234		

(continued)

TABLE 4.6 (continued)

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Women not living with own child(ren), including those who did not have any				
Received GED by end of month 24 (%)	31.1	13.0	18.1***	0.000
Received GED or high school diploma by end of month 24 (%)	32.4	18.2	14.2***	0.000
Received trade certificate or license by end of month 24 (%)	16.2	5.7	10.6***	0.000
Received associate's or 2-year college degree by end of month 24 (%)	0.4	0.0	0.3	0.415
Received bachelor's or 4-year college degree by end of month 24 (%)	0.0	0.0	0.0	1.000
Sample size	261	223		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the mo[illegible]f random assignment may be less than a month, beginning with the date of random assignment [illegible]d ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

GED examination for both experimentals and controls. By month 24 of follow-up, 16.4 percent of all controls had received a high school diploma or passed the GED examination, with about equal proportions among the three subgroups in Table 4.6. Among experimentals, 33.2 percent had attained a high school diploma or GED, with about equal percentages for experimentals among men, women living with their own children, and other women.

Thus, the full-sample impact on attainment of a GED or high school diploma by month 24 was 16.7 percentage points, which compares very favorably to the results of the residential Job Corps evaluation. The impact was 15.2 percentage points among men, 21.3 percentage points among women living with their own children, and 14.2 percentage points among other women.

Compared to the impacts just described for attainment of a diploma or GED, the impact on attainment of a GED alone was slightly larger. This was the case because controls (who were not as a rule participating in a special alternative program such as JOBSTART) were slightly more likely to return to regular high school than were experimentals, although both events were rare. The impact on passing of the GED examination was 18.9 percentage points for the full sample, 16.8 percentage points among men, 23 percentage points among women living with their own children, and 18.1 percentage points among other women.[13]

These large educational attainment impacts were present for many different subgroups in the sample, as shown in Table 4.7 for passing of the GED examination.[14] For example, the first three rows of the table present impacts on GED receipt for women and men. For women, impacts were 20.8 percentage points, while for men they were 16.8 percentage points. The difference in impacts for these two groups, 4 percentage points, is reported in the column labeled "subgroup impact difference," and is not statistically significant. Other subgroup findings include a 23.2 percentage point impact on passing of the GED examination for women living with their own children, 17.3 percentage points for youths who quit school before the

---

[13] The JOBSTART survey also asked about receipt of a trade certificate or license during the follow-up period. The precise meaning of these certificates and licenses is not clear. Some could have been awarded for completion of a program, rather than for a generally recognized occupational competency. Nevertheless, 19.2 percent of experimentals received such certificates and licenses, compared to 8.9 percent of controls, for an impact of 10.3 percentage points. The impact size was slightly higher than this for men and about half this for women living with their own children.

[14] The results presented in this paragraph are based on an analysis similar to that used for Table 4.4. That is, impacts are for subgroups designated by the named characteristic, with differences in other observed characteristics statistically controlled for through linear regression.

TABLE 4.7

IMPACTS ON GED ATTAINMENT THROUGH MONTH 24, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

Characteristic and Subgroups	Sample Size	Received GED by End of Month 24		Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
		Experimentals	Controls				
Gender						4.0	0.270
Women	968	33.3%	12.5%	20.8***	0.000	---	---
Men	871	27.8	11.0	16.8***	0.000	---	---
Ethnicity						---	0.371
White, non-Hispanic	155	42.5	24.6	17.9***	0.004	---	---
Black, non-Hispanic	840	30.1	11.9	18.2***	0.000	---	---
Hispanic	783	29.9	8.9	21.0***	0.000	---	---
Other	61	18.1	15.1	3.0	0.766	---	---
Ethnicity, by gender						---	0.611
Women							
White, non-Hispanic	84	49.5	24.6	24.9***	0.003	---	---
Black, non-Hispanic	445	33.2	12.3	20.9***	0.000	---	---
Hispanic	413	31.6	10.8	20.8***	0.000	---	---
Other	26	13.5	9.3	4.2	0.787	---	---
Men							
White, non-Hispanic	71	33.8	24.7	9.1	0.329	---	---
Black, non-Hispanic	395	26.6	11.3	15.2***	0.000	---	---
Hispanic	370	27.9	6.8	21.1***	0.000	---	---
Other	35	22.2	17.9	4.3	0.746	---	---
Parental status						---	0.529
Women living with own child(ren)							
No	484	32.8	14.5	18.3***	0.000	---	---
Yes	484	33.3	10.1	23.2***	0.000	---	---
Men who have own child(ren)							
No	765	28.5	11.5	17.0***	0.000	---	---
Yes	106	25.0	9.7	15.3**	0.046	---	---

(continued)

TABLE 4.7 (continued)

Characteristic and Subgroups	Sample Size	Received GED by End of Month 24		Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
		Experimentals	Controls				
Employed within past year						-11.4***	0.002
No	870	27.0%	14.1%	12.8***	0.000	---	---
Yes	969	34.1	9.8	24.3***	0.000	---	---
Prior employment, by gender						---***	0.001
Women employed within past year							
No	547	30.8	13.2	17.7***	0.000	---	---
Yes	421	36.2	11.4	24.8***	0.000	---	---
Men employed within past year							
No	323	21.4	16.9	4.6	0.295	---	---
Yes	548	31.8	8.0	23.8***	0.000	---	---
Left school in grade 11 or 12						2.7	0.458
No	1,078	29.9	9.9	20.0***	0.000	---	---
Yes	761	31.9	14.6	17.3***	0.000	---	---
Received occupational training within past year						-7.9	0.102
No	1,529	30.2	12.7	17.5***	0.000	---	---
Yes	310	33.6	8.1	25.4***	0.000	---	---
Age						-3.4	0.412
16-19	1,359	30.9	12.9	18.0***	0.000	---	---
20 or 21	480	30.2	8.8	21.4***	0.000	---	---
Age, by gender						---	0.594
Women							
16-19	710	33.4	13.4	20.1***	0.000	---	---
20 or 21	258	33.2	10.4	22.8***	0.000	---	---
Men							
16-19	649	28.0	12.2	15.7***	0.000	---	---
20 or 21	222	26.9	7.1	19.8***	0.000	---	---

(continued)

TABLE 4.7 (continued)

Characteristic and Subgroups	Sample Size	Received GED by End of Month 24		Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
		Experimentals	Controls				
Marital status						16.8***	0.007
Ever married	174	40.7%	6.6%	34.0***	0.000	---	---
Never married	1,665	29.7	12.4	17.3***	0.000	---	---
Living in own household or with boy/girlfriend						-2.4	0.608
No	1,500	29.9	11.5	18.5***	0.000	---	---
Yes	339	34.2	13.4	20.9***	0.000	---	---
Own AFDC case or receiving General Assistance						1.1	0.788
No	1,344	29.5	10.3	19.2***	0.000	---	---
Yes	495	34.1	16.0	18.1***	0.000	---	---
Own AFDC case						0.1	0.984
No	1,446	31.3	12.4	18.9***	0.000	---	---
Yes	393	28.5	9.7	18.8***	0.000	---	---
Receiving Food Stamps						-1.2	0.752
No	1,143	29.5	11.0	18.4***	0.000	---	---
Yes	696	32.8	13.2	19.6***	0.000	---	---
Arrested since age 16						-1.5	0.772
No	1,567	30.6	11.9	18.6***	0.000	---	---
Yes	272	31.5	11.3	20.1***	0.000	---	---
Lived with both parents at age 14						3.2	0.397
No	1,198	31.2	11.2	20.0***	0.000	---	---
Yes	641	29.8	13.0	16.8***	0.000	---	---

(continued)

TABLE 4.7 (continued)

Characteristic and Subgroups	Sample Size	Received GED by End of Month 24		Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
		Experimentals	Controls				
Reason for leaving regular high school						---	0.440
School-related	886	31.0%	12.9%	18.1***	0.000	---	---
Job-related	182	29.1	16.0	13.1**	0.025	---	---
Other	771	30.6	9.6	21.0***	0.000	---	---

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.

eleventh grade, and 18.8 percentage points for youths with their own AFDC case at random assignment.

CHAPTER 5

IMPACTS ON EMPLOYMENT, EARNINGS, AND OTHER OUTCOMES

Chapter 4 showed that JOBSTART had strong impacts on completion of high school or receipt of a GED for most subgroups of the full sample, and that these strong impacts remained large over time. This chapter focuses on the extent to which the impact on educational attainment has, to this point, translated into increased labor market success and less reliance on public benefit programs. The following key questions about labor market and other outcomes are addressed:

- What sacrifice of employment opportunities or earnings did experimentals make during the first year after random assignment, which for many was primarily a period spent in JOBSTART?
- Had the experimentals begun to catch up with the controls in employment and earnings by the end of the second year?
- How did experimentals and controls compare in earnings and employment within important subgroups? Were the labor market effects of JOBSTART different for men, young mothers, and other women? Did key JOBSTART impacts vary according to age, grade at time of dropout, or other characteristics of the young people in the sample?
- Do more precise measures of work effort and further information on wages earned shed light on the basic employment rate and earnings impacts that were observed during the follow-up period?
- What effect did the program have on other outcomes such as time spent in productive activity (that is, employment, education, or training), receipt of public benefits, criminal activity, and childbearing?

The findings described here, based on two years of follow-up, are interim results; they represent the early post-program period for most of the sample. The analysis in this chapter does not attempt any projections of employment, earnings, or other outcomes into the future. The third wave of the follow-up survey, fielded at approximately four years after random assignment of the sample members, will extend the data for an additional two years.

A summary of this chapter may prove useful at the outset. The analysis focuses on the full sample and three key subgroups: mothers, other women, and men. In brief: The impacts on employment and earnings were encouraging for mothers; small, but slightly positive, for other women; and generally negative throughout the period for men. The analysis presents impacts on employment and total earnings, using simple measures, and then offers possible explanations for the basic results.

In more detail: After an initial period of activity in JOBSTART, experimentals in the full sample gradually caught up with controls in employment rate. For young mothers and other women, the rate slightly exceeded that of their control group counterparts by the second year of follow-up, while men drew approximately even with controls. For the full sample over the entire 24-month period, experimentals remained below controls in cumulative earnings, but there were important differences among the key subgroups. Young mothers in the experimental group earned more than controls during the 24-month period; other women in the experimental group earned more than controls during the second 12-month period, nearly canceling out their first-year earnings loss. The earnings of male controls substantially exceeded those of male experimentals in both years.

Since earnings are a function of hours and wages, analyzing these more refined measures of labor market success helps explain the basic earnings impact findings. Despite the employment rates of experimentals and controls being nearly equal during the second year of follow-up, controls remained ahead in actual time spent working, although again there were differences among the key subgroups. Throughout most of the follow-up period, a higher proportion of experimentals than controls were attending education or training classes, which may explain, or partly explain, why experimentals spent less time working. In other words, investment in human capital continued throughout the follow-up period, supporting the view that the final story on employment impacts cannot yet be told.

I. JOBSTART as an Investment

If programs like JOBSTART are effective, they will lead to long-run gains in employment and earnings, but in the short run there may be negative impacts. During the first year after entering the sample, JOBSTART youths had less time available for work than did controls, since for much of that period, they were in an intensive program of education and skills instruction. Chapter 3 showed that more than 50 percent of experimentals were still active in the sixth month after entering the sample; 16 percent were still active in the twelfth month; and almost 10 percent were still active in the fifteenth month. Throughout most of the two-year follow-up period, a larger percentage of experimentals than controls were participating in a program. Controls, therefore, got a head start in the labor market, since experimentals could not be in JOBSTART and be working during any given hour of the day. Also, for young people with poor skills, work experience itself can be an important source of new job skills and higher wages.

GED attainment during or after intensive JOBSTART education ultimately may open up many employment opportunities for JOBSTART graduates. But even after a sample member left JOBSTART, it might still take some time to become as well-settled in the labor market as his or her control group counterpart, who might have been learning new skills while working. Thus, well into the second year of follow-up, controls might still be expected to have had the edge on experimentals in employment and earnings.

The evaluation of the residential Job Corps provides an example of this point.[1] That study began its follow-up at the point of termination from the program, and it reported impacts only for six-month intervals rather than for individual months. For the period from program termination to six months thereafter, the employment and earnings of those in the program group were slightly lower than those of the comparison group (although the difference was not statistically significant). The employment and earnings rates of the program group began to exceed those of the comparison group six to 12 months following termination from the program, and they continued to be higher throughout the remainder of the 48-month follow-up period.

These findings cannot be applied directly to the JOBSTART evaluation because follow-up in JOBSTART began at random assignment – the point when youths were ready to enter the program. However, since the average length of stay in JOBSTART was roughly six months, the 12-month survey was conducted about six months after the average sample member left the program, and the 24-month survey was conducted about 18 months after exit from the program. In sum, the monthly employment and earnings impacts in the Job Corps study turned positive during a period roughly corresponding to the second year of follow-up covered by this report.

---

[1] See Mallar et al., 1982, p. 135.

II. JOBSTART's Impacts on Employment Rates

Because young adults tend to apply for employment and training programs when they are between jobs, the employment rate grew over time for both experimentals and controls (Table 5.1). For the full sample, a larger fraction of controls than experimentals was employed in each month of the first year.[2] The difference in employment rate peaked in month five and then mainly declined over time. After month 12, the employment rate of experimentals was greater than that of controls more often than it was below it, but in no month was this difference statistically significant. About 46 percent of each group worked at some point during the last month of follow-up (month 24). During the second year as a whole, 69.5 percent of controls worked at some point, as did 72.0 percent of experimentals, for a positive employment rate impact of 2.5 percentage points. This impact was, however, not statistically significant.

A. Impacts by Gender

As explained in Chapter 4, many previous evaluations of youth employment and training programs have found better program effects for women than for men.[3] One likely explanation for women's better employment results is that it is easier to *improve* the employment and earnings of those who do not spend much time in the world of work (for example, young mothers) than of those who are already in the labor force but fail to find and keep steady, well-paying jobs (for example, young men with poor skills). Thus, from this perspective, women have greater potential for improved labor market outcomes than do men, and less to lose (in terms of forgone employment and earnings) by investing in education and training.

The JOBSTART sample exemplifies this pattern. During the first year after random

---

[2] The appearance of a sharp increase in employment rates in month 13 is very likely due to survey measurement error. As explained in Appendix A, 235 of the 1,839 24-month survey completers responded to a single combination instrument covering their labor market behavior over the entire two-year period, while 1,604 of them responded to separate survey efforts 12 and 24 months after random assignment. Separate analysis of the 235 "combination" responders showed continuous growth in employment rates over the 24-month follow-up period. But many in the sample who responded to a survey after 12 and 24 months showed a discontinuity in employment rates at month 13 owing to poor and inconsistent recall. For example, respondents to the 24-month survey reported employment at the start of the period covered by the survey (month 13) that probably started somewhat later. This happened even though the survey staff prompted respondents with their previous responses to questions about employment in month 12.

[3] See Betsey et al., 1985, for a survey.

TABLE 5.1

IMPACTS ON EMPLOYMENT RATES THROUGH MONTH 24

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Ever employed				
Months 1-24	79.2%	77.9%	1.3	0.474
Months 1-12	57.6	61.6	-4.0*	0.061
Months 13-24	72.0	69.5	2.5	0.212
Quarter 1	20.5	29.8	-9.4***	0.000
Quarter 2	30.4	40.1	-9.7***	0.000
Quarter 3	40.7	44.2	-3.5	0.111
Quarter 4	48.1	49.6	-1.5	0.501
Quarter 5	54.6	52.7	1.8	0.397
Quarter 6	48.1	47.9	0.2	0.940
Quarter 7	50.5	50.6	-0.1	0.954
Quarter 8	53.4	51.7	1.6	0.453
Ever employed				
Month 1	13.1	20.4	-7.3***	0.000
Month 2	16.5	24.4	-7.9***	0.000
Month 3	18.5	27.0	-8.5***	0.000
Month 4	21.1	30.3	-9.2***	0.000
Month 5	23.6	33.1	-9.5***	0.000
Month 6	26.5	34.0	-7.4***	0.000
Month 7	30.3	35.7	-5.3**	0.011
Month 8	32.2	37.2	-5.0**	0.019
Month 9	35.3	38.3	-3.0	0.159
Month 10	38.5	39.6	-1.2	0.592
Month 11	38.1	41.8	-3.7*	0.089
Month 12	41.0	42.2	-1.3	0.562
Month 13	47.7	45.4	2.4	0.281
Month 14	39.2	36.4	2.8	0.187
Month 15	41.2	39.3	1.9	0.385
Month 16	41.6	40.0	1.6	0.461
Month 17	41.2	42.1	-0.9	0.684
Month 18	42.3	43.3	-1.0	0.645
Month 19	42.3	44.1	-1.8	0.403
Month 20	42.9	43.7	-0.8	0.717
Month 21	44.8	44.0	0.8	0.726
Month 22	46.2	44.5	1.7	0.441
Month 23	45.6	45.8	-0.2	0.941
Month 24	46.1	46.7	-0.7	0.763
Sample size	949	890		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

(continued)

TABLE 5.1 (continued)

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

assignment, 74.8 percent of control men worked, compared to 38.3 percent of women living with their own children and 61.5 percent of other women (Table 5.2).[4] During this period, which, for many experimentals, was largely a time of program participation, the impact of JOBSTART on employment rates was 2.7 percentage points for the young mothers, -4.5 percentage points for other women, and -8.1 percentage points for men. Thus, JOBSTART entailed an opportunity cost (forgone employment) for men and women who were not caring for children, but not for young mothers.

A second possible explanation for the poorer impacts observed for men in JOBSTART is the greater difficulty of placing men in jobs that value a GED. Many women in JOBSTART, for example, were trained in clerical occupations and sought clerical jobs, for which educational credentials were important. However, young men were more likely to train for work in occupations that did not, at least initially, value a GED – for example, in many types of blue-collar work, especially physically demanding jobs.[5] Further, it may have been harder to find training-related jobs for men who did study for occupations in which having a GED matters. These explanations for women's stronger impacts will be explored in future work on the JOBSTART Demonstration. So far (during two years of follow-up), the investment in JOBSTART services follows the usual pattern and has not paid off for men in higher employment rates. Although they regained the ground they lost in the first year, they did not surpass the controls in the second year. Table 5.2 shows that the impact on employment rates was precisely zero for men during the second year of follow-up; exactly 84.3 percent of male experimentals and controls worked at some point during those 12 months.

In contrast, female experimentals – especially young mothers – seem to have overtaken female controls in employment during the same period. Table 5.2 shows that the impact on employment rates for young mothers was 7.2 percentage points during months 13 through 24, and for other women it was 4.2 percentage points; these impacts were not, however, statistically significant.

The month-by-month story is less clear than that told by cumulative yearly employment rates. Figure 5.1 shows monthly employment rates for experimentals and controls among men,

---

[4] In the rest of this analysis, women living with their own children will be referred to as "mothers" even though some of the "other women" may be mothers who were not living with their children.

[5] Chapter 3 reported on the occupations for which men and women trained and showed a much higher percentage of women studying for clerical occupations.

TABLE 5.2

IMPACTS ON EMPLOYMENT RATES THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Ever employed (%)				
Months 1-24	89.0	90.1	-1.1	0.583
Months 1-12	66.7	74.8	-8.1***	0.006
Months 13-24	84.3	84.3	0.0	0.990
Sample size	438	433		
Women living with own child(ren)				
Ever employed (%)				
Months 1-24	62.1	57.2	4.9	0.279
Months 1-12	41.0	38.3	2.7	0.543
Months 13-24	53.3	46.1	7.2	0.113
Sample size	250	234		
Women not living with own child(ren), including those who did not have any				
Ever employed (%)				
Months 1-24	79.3	75.8	3.4	0.363
Months 1-12	57.0	61.5	-4.5	0.311
Months 13-24	69.5	65.3	4.2	0.332
Sample size	261	223		

SOURCES FOR TABLE 5.2 AND FIGURE 5.1: MDRC calculations from JOBSTART enrollment form and survey data (Table 5.2); Appendix Table F.5 (Figure 5.1).

NOTES FOR TABLE 5.2 AND FIGURE 5.1: Calculations for this table and figure used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

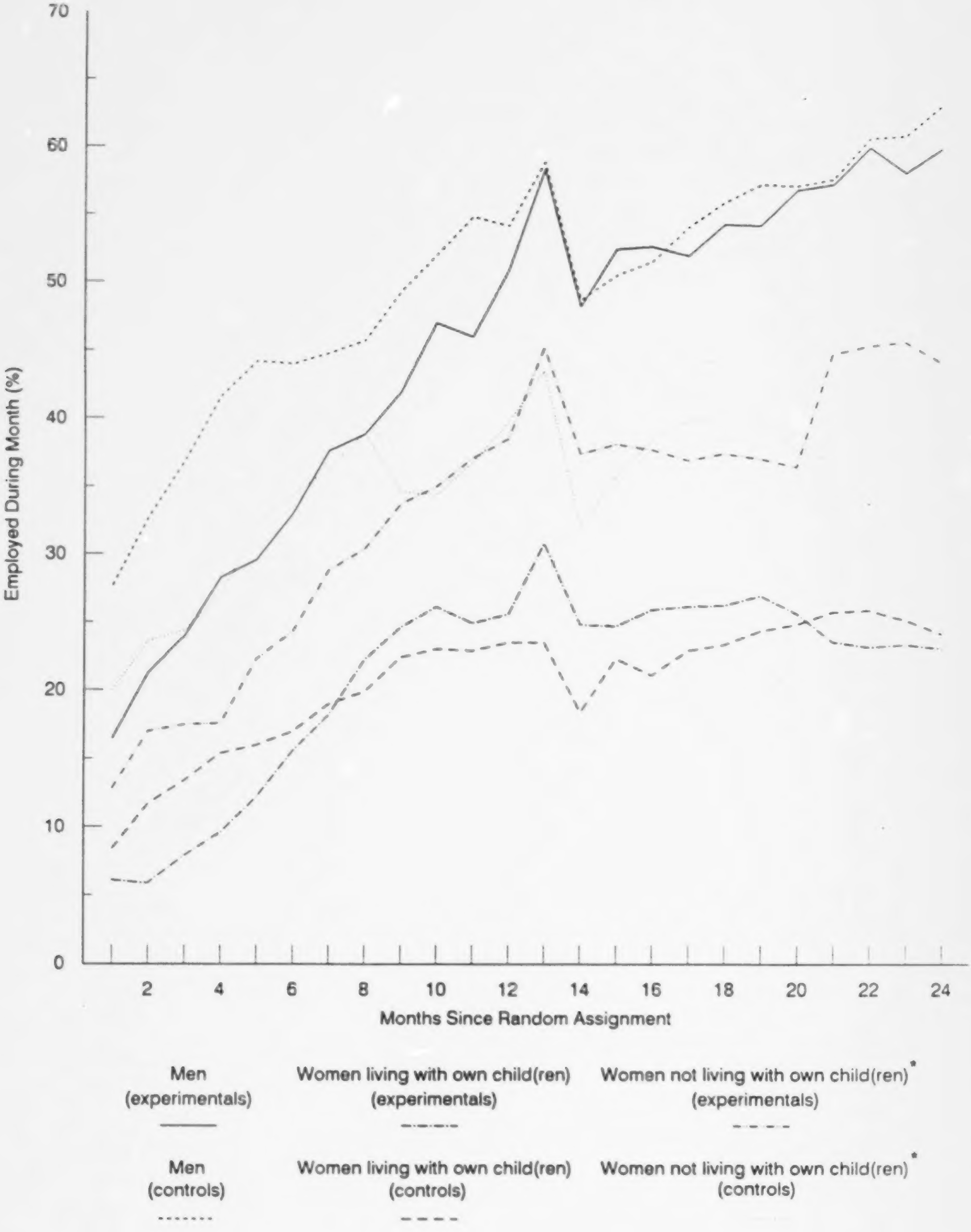
Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

FIGURE 5.1

MONTHLY EMPLOYMENT RATES, BY GENDER AND PARENTAL STATUS



*Includes women who did not have children.

young mothers, and other women. Young mothers who were in the experimental group appear to have caught up with, and sometimes overtaken, their control group counterparts in monthly employment rates part-way through the first year, while other women in the experimental group caught up with their control group counterparts at about the beginning of the second year. For the "other women" subgroup, there also appears to have been a trend toward growing positive impacts in the last six months of the follow-up period. In contrast, experimental men remained slightly behind control men during most of the second year. Thus, although the jury is still out on employment rate impacts, the findings certainly are more encouraging for women than for men.

B. Impacts for Other Subgroups

In addition to looking at impacts for subgroups separately (for example, looking at the impacts for men, women living with their own children, and other women), it is useful to estimate employment rate impacts for subgroups controlling for differences in characteristics other than the one used to designate the groups (for example, gender).[6] This helps explain whether any observed differences in impacts for men, women living with their own children, and other women were due to other factors correlated with gender and parenting status (such as past employment experience) or continue to be present even after such differences are controlled for in the analysis. This type of analysis also allows calculation of whether differences between impacts for women and men were statistically significant.

Table 5.3 addresses these issues by presenting impacts on the percentage of the sample ever employed during months 13 through 24. Each section of the table (such as the first for women and men) presents impacts for the designated subgroups, along with the between-subgroups impact difference.[7] The entry to the right of the first line of the table

---

[6] Chapter 2 highlighted ways in which JOBSTART women were different from JOBSTART men in characteristics besides gender. The impact estimates for men, young mothers, and other women, in Table 5.2 were done by splitting the entire sample into three subsamples. To make the estimates more precise, the separate estimates for each group took into account small observed differences between experimentals and controls in each subsample through regression adjustments. But there were many differences among the three subsamples in JOBSTART other than their gender and parenting status; for example, women – especially young mothers – had less work experience than did men. This makes it difficult to understand the sources of differences in impacts among the three groups.

[7] The impact estimates in this table were produced by conducting a two-way analysis of covariance controlling for differences in pre-random assignment characteristics other than the characteristic used to (continued...)

TABLE 5.3

IMPACTS ON YEAR-TWO EMPLOYMENT RATES,
BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

		Ever Employed, Months 13-24					
Characteristic and Subgroups	Sample Size	Experimentals	Controls	Subgroup Impact	p^{a}	Subgroup Impact Difference^{b}	p^{a}
Gender						4.9	0.222
Women	968	64.7%	60.0%	4.8*	0.081	---	---
Men	871	80.0	80.1	-0.1	0.977	---	---
Ethnicity						---	0.203
White, non-Hispanic	155	82.9	78.8	4.1	0.550	---	---
Black, non-Hispanic	840	69.3	64.1	5.3*	0.073	---	---
Hispanic	783	73.9	73.2	0.7	0.820	---	---
Other	61	55.3	72.8	-17.6	0.110	---	---
Ethnicity, by gender						---	0.382
Women							
White, non-Hispanic	84	78.4	70.1	8.3	0.369	---	---
Black, non-Hispanic	445	64.0	54.2	9.8**	0.015	---	---
Hispanic	413	64.4	64.6	-0.3	0.950	---	---
Other	26	40.4	55.8	-15.4	0.364	---	---
Men							
White, non-Hispanic	71	87.2	88.6	-1.4	0.893	---	---
Black, non-Hispanic	395	74.7	74.7	-0.0	0.995	---	---
Hispanic	370	84.7	82.9	1.7	0.692	---	---
Other	35	70.8	87.5	-16.7	0.255	---	---
Parental status						---	0.660
Women living with own child(ren)							
No	484	67.0	63.0	4.0	0.309	---	---
Yes	484	58.5	52.9	5.6	0.148	---	---
Men who have own child(ren)							
No	765	82.7	82.6	0.1	0.986	---	---
Yes	106	79.2	80.3	-1.1	0.895	---	---

(continued)

TABLE 5.3 (continued)

Characteristic and Subgroups	Sample Size	Ever Employed, Months 13-24 Experimentals	Controls	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Employed within past year						3.2	0.424
No	870	70.0%	65.8%	4.2	0.150	---	---
Yes	969	73.8	72.8	1.0	0.725	---	---
Prior employment, by gender							
Women employed within past year						---	0.414
No	547	61.1	57.0	4.1	0.258	---	---
Yes	421	68.2	62.6	5.6	0.182	---	---
Men employed within past year							
No	323	80.3	76.0	4.2	0.377	---	---
Yes	548	80.7	83.4	-2.6	0.470	---	---
Left school in grade 11 or 12						-3.0	0.454
No	1,078	68.9	67.7	1.2	0.637	---	---
Yes	761	76.3	72.1	4.3	0.169	---	---
Received occupational training within past year						6.1	0.247
No	1,529	72.3	68.7	3.5	0.104	---	---
Yes	310	70.6	73.2	-2.6	0.592	---	---
Age						0.0	0.999
16-19	1,359	72.7	70.3	2.5	0.285	---	---
20 or 21	480	69.9	67.4	2.5	0.527	---	---
Age, by gender						---	0.681
Women							
16-19	710	65.5	60.5	5.0	0.122	---	---
20 or 21	258	62.9	58.5	4.3	0.412	---	---
Men							
16-19	649	80.8	81.0	-0.2	0.952	---	---
20 or 21	222	77.7	77.4	0.3	0.955	---	---

(continued)

TABLE 5.3 (continued)

Characteristic and Subgroups	Sample Size	Ever Employed, Months 13-24				Subgroup Impact Difference[b]	p[a]
		Experimentals	Controls	Subgroup Impact	p[a]		
Marital status						-0.3	0.969
Ever married	174	77.6%	75.3%	2.2	0.729	---	---
Never married	1,665	71.4	68.9	2.5	0.231	---	---
Living in own household or with boy/girlfriend						-0.8	0.869
No	1,500	72.2	69.9	2.3	0.295	---	---
Yes	339	71.1	67.9	3.1	0.496	---	---
Own AFDC case or receiving General Assistance						5.4	0.229
No	1,344	74.1	70.2	3.9*	0.092	---	---
Yes	495	66.3	67.7	-1.5	0.700	---	---
Own AFDC case						1.2	0.810
No	1,446	73.0	70.3	2.7	0.224	---	---
Yes	393	68.1	66.6	1.6	0.717	---	---
Receiving Food Stamps						4.9	0.231
No	1,143	73.3	68.9	4.3*	0.085	---	---
Yes	696	69.9	70.5	-0.6	0.859	---	---
Arrested since age 16						-8.4	0.136
No	1,567	72.1	70.9	1.2	0.569	---	---
Yes	272	71.2	61.6	9.6*	0.064	---	---
Lived with both parents at age 14						5.7	0.170
No	1,198	71.2	66.8	4.5*	0.069	---	---
Yes	641	73.4	74.6	-1.2	0.710	---	---
Reason for leaving regular high school						---	0.411
School-related	886	74.0	69.1	4.9*	0.085	---	---
Job-related	182	73.8	77.3	-3.5	0.585	---	---
Other	771	69.1	67.9	1.2	0.702	---	---

(continued)

TABLE 5.3 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.

shows that – controlling for other measured differences in the subgroups – the impact for women was 4.9 percentage points greater than the impact for men and that this "subgroup impact difference" was not statistically significant.[8] The "subgroup impact" column shows that this difference is calculated as 4.8 percentage points (the impact for the women) less -0.1 percentage points (the impact for men). The within-subgroup impact was statistically significant for women but not for men.[9] The more detailed categories based on parenting status by gender also show stronger impacts for women than for men, and for mothers than for other women, although here again the differences in impacts were not statistically significant. Thus, the earlier apparent differences in employment rate impacts for men, young mothers, and other women *did not* disappear (although they were not statistically significant) when other measured characteristics of each group were accounted for in the analysis.[10]

The rest of Table 5.3 presents other subgroup impacts on second-year employment rates in a similar fashion. Nothing in the final column is significant in a statistical sense, although impact differences seem sizable in absolute terms (more than 5 percentage points) for arrest history, prior training, two-parent upbringing, welfare receipt, and ethnicity.

There is no clear pattern to these differences in impacts.

- In some of these cases, those with greater barriers to employment (represented by control group employment rates) had greater impacts. Women had larger impacts than men, youths with no previous training had larger impacts than those with previous training, those arrested since

---

[7](...continued)
define the subgroup under review. See Ostle, 1975, p. 461. The adjustments were done using a linear regression model. Characteristics that affect outcomes and impacts through a nonlinear relationship are not controlled for with this procedure. And no such procedure could control for unmeasured characteristics that affected outcomes and impacts. The adjusted outcomes and impacts presented here for women and for men are not identical to those presented in Table 5.2 because the former attempt to take observed non-gender differences between women and men into account. The differences in impacts are very minor, but the adjustments change some outcome measures several percentage points.

[8] This is indicated by the absence of stars on the "subgroup impact difference."

[9] This subgroup impact can be statistically significant, even though it is smaller than the impact for young mothers alone, because the sample size is larger.

[10] As discussed in Chapter 2 and Appendix B, subgroup impact differences must be interpreted carefully because characteristics such as gender were not assigned at random and so can be correlated with other characteristics. It could very well be that it was not gender per se that explained apparently higher impacts for women than for men, but some observed or unobserved non-gender difference between the women and the men in this sample. The calculations in Table 5.4 incorporate simple adjustments for observed characteristics besides gender, but not all differences were observed, and observed characteristics may have had more complex effects on outcomes and impacts than those modeled in these adjustments.

age 16 had larger impacts than those who had not been arrested, and those who did not live with two parents at age 14 had larger impacts than those living in a two-parent family.

- For other subgroups, however, the opposite pattern emerged. Impacts were higher for those not receiving public assistance at random assignment than for recipients.

- For some subgroups – most notably those based on ethnicity – no clear pattern emerged. Blacks had the highest impacts, Hispanics had virtually no impacts, and "other nonwhites" (primarily Asians) had large negative impacts.

This mixed finding differs from the results of several studies of employment programs for welfare recipients, which did find patterns among subgroup impacts. In these programs, impacts tended to be small or nonexistent for the most job-ready and the least job-ready and positive for a group in the middle tier.[11] Clearly, the subgroups used in the JOBSTART analysis are much more narrowly defined, which may contribute to the absence of a pattern in the impacts.[12]

Other characteristics seem to have been more weakly associated with impact differences in second-year employment rates:

- **Recent employment.** Employment at some point in the year before enrollment is associated with a 3.2 percentage point lower impact, weakly supporting the position that it is easier to raise employment rates for those who have less of a work history.

- **Recent employment, by gender.** Employment rate impacts were positive both for women who had some employment and for women who had no employment in the year before program entry.[13] However, for men, the positive impacts for those who were not employed were canceled out by negative impacts for those who had some employment in the pre-program year.

---

[11] See Friedlander, 1988.

[12] A second reason for not highlighting differences in impacts for subgroups is that the findings are somewhat sensitive to the outcome measure chosen. For example, when a similar analysis is done for employment in the last three months of follow-up, the results are somewhat different, but the general conclusion (that there is no clear pattern to the subgroup findings) remains.

[13] For the previously employed women, the impact in year two was 1.5 percentage points higher than the impact for women who were not employed, although this subgroup impact difference was not statistically significant.

- **Highest grade attended.** While all JOBSTART enrollees were high school dropouts, some left school in the ninth grade, while others dropped out during their senior year. Although the difference in impacts was not statistically significant, second-year employment was improved by a wider margin for those who dropped out in later grades than for those who dropped out earlier.[14]

- **Recent prior skills training.** For a substantial minority of the sample, JOBSTART was not the first try at a second-chance program. The second-year employment impact was 6.1 percentage points higher (not statistically significant) for those who had not tried another program during the 12 months preceding enrollment in JOBSTART.

- **Age.** Very different patterns of labor market behavior are shown at each age in the general youth population. Labor force participation, employment, and earnings increase dramatically from age 16 to the early twenties. Moreover, program operators often suggest that younger enrollees do not derive as much benefit from training as do somewhat older youths who are ready to "settle down" and pursue stable employment. Table 5.3 gives no support whatever to this hypothesis: The difference in impacts between teenagers and older youths is precisely zero and the younger youths have higher employment rates.[15]

III. JOBSTART's Impacts on Earnings

The full-sample impact of JOBSTART on earnings during the two-year follow-up period is less encouraging than on employment rates, although once again there is some evidence of movement in the right direction. As shown in Table 5.4, during the first 12 months following random assignment, controls earned $585 more than experimentals, while during the second 12 months, controls earned $205 more. Once again, the month-by-month pattern of impacts is less clear than the pattern of annual impacts: While experimentals moved even with controls during months 14 and 15, after that point the earnings impact fluctuated within a fairly narrow and slightly negative range.

---

[14] This difference was driven by lower employment for controls who dropped out earlier rather than by higher employment for experimentals who dropped out earlier.

[15] On this point, analysis of the employment rate for the last three months of follow-up suggests a different result: Younger women do better than older women, while older men do better than younger men.

TABLE 5.4

IMPACTS ON EARNINGS THROUGH MONTH 24

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Total earnings				
Months 1-24	$5,859.56	$6,649.07	-$789.52***	0.010
Months 1-12	1,965.47	2,550.26	-584.78***	0.000
Months 13-24	3,894.08	4,098.81	-204.73	0.331
Earnings				
Month 1	37.92	64.30	-26.39***	0.001
Month 2	93.65	141.78	-48.13***	0.000
Month 3	109.16	165.51	-56.35***	0.000
Month 4	125.41	187.20	-61.79***	0.000
Month 5	146.31	211.13	-64.81***	0.000
Month 6	158.06	222.28	-64.22***	0.000
Month 7	179.79	234.87	-55.08***	0.001
Month 8	196.91	240.63	-43.72**	0.011
Month 9	213.35	256.66	-43.31**	0.013
Month 10	228.26	265.40	-37.15**	0.036
Month 11	226.07	281.29	-55.22***	0.002
Month 12	250.59	279.20	-28.61	0.115
Month 13	254.54	273.39	-18.85	0.289
Month 14	294.66	291.32	3.33	0.870
Month 15	313.56	309.39	4.17	0.839
Month 16	314.47	324.95	-10.48	0.609
Month 17	317.17	340.33	-23.16	0.269
Month 18	322.40	346.92	-24.52	0.239
Month 19	320.80	364.48	-43.68**	0.041
Month 20	336.01	357.61	-21.60	0.320
Month 21	351.86	360.39	-8.53	0.695
Month 22	354.24	370.68	-16.44	0.445
Month 23	355.78	373.41	-17.63	0.417
Month 24	358.60	385.95	-27.35	0.214
Sample size	949	890		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

(continued)

TABLE 5.4 (continued)

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

A. Impacts by Gender

As with employment rates, the impacts for earnings at this point seem more encouraging for women than for men. Table 5.5 shows that for men, controls earned $1,028 more than experimentals in the first year of follow-up and $667 more in the second.[16] For women who were living with their own children, experimentals have already pulled slightly ahead of controls; experimentals earned $89 less in the first year and then earned $172 more in the second year. The trend for other women is also in a positive direction: An earnings loss of $423 was followed by an earnings gain of $202.

B. Impacts for Other Subgroups

Table 5.6 presents second-year earnings impacts by selected subgroup and differences among these subgroup impacts.[17] As in Table 5.3, this table presents comparisons of subgroup impacts for the designated groups, controlling for differences other than the one used to define the subgroup categories. The first line of the table shows that the difference in second-year earnings impact by gender (controlling for other measured differences of men and women) was $826 (-$639 for men versus $187 for women) and that it was statistically significant. The observed male-female earnings differences in impacts, presented in Table 5.5, therefore, do not appear to have resulted from other measured non-gender differences in their pre-random assignment characteristics (such as differences in education or work experience.)

The rest of Table 5.6 shows impacts by other subgroup splits. The results generally mirror what already has been said about employment rates, with a few exceptions.

IV. Moving Behind These Employment-Related Impacts

The analysis presented so far can be summarized as showing that employment rates for experimentals have caught up with those of controls during the follow-up period, but (especially for men) earnings of experimentals still lag behind. A starting point for understanding this result is to look behind the summary measure of employment success

---

[16] Male controls also out-earned experimentals in every month of the follow-up period, though during much of the second year, these differences were not statistically significant.

[17] As in Table 5.4, adjusted outcomes and impacts by gender differ slightly from their counterparts in previous tables because of the different way they were calculated, using the entire sample rather than splitting by gender and making separate calculations.

TABLE 5.5

IMPACTS ON EARNINGS THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p^a
Men				
Total earnings ($)				
2-year total	7,797.22	9,492.61	-1,695.39***	0.001
Year 1	2,648.50	3,676.88	-1,028.38***	0.000
Year 2	5,148.72	5,815.73	-667.01*	0.057
Quarter 1	348.17	551.11	-202.93***	0.001
Quarter 2	581.48	918.15	-336.67***	0.000
Quarter 3	796.52	1,022.11	-225.60***	0.008
Quarter 4	922.33	1,185.50	-263.18***	0.003
Quarter 5	1,119.86	1,286.81	-166.94*	0.075
Quarter 6	1,266.70	1,426.00	-159.30	0.112
Quarter 7	1,354.56	1,506.69	-152.13	0.134
Quarter 8	1,407.60	1,596.24	-188.64*	0.063
Sample size	438	433		
Women living with own child(ren)				
Total earnings ($)				
2-year total	3,035.55	2,952.31	83.24	0.852
Year 1	1,011.83	1,100.96	-89.13	0.647
Year 2	2,023.73	1,851.35	172.38	0.600
Quarter 1	75.56	151.45	-75.89**	0.036
Quarter 2	200.41	247.18	-46.77	0.396
Quarter 3	336.41	328.49	7.93	0.905
Quarter 4	399.44	373.84	25.60	0.733
Quarter 5	474.35	355.07	119.27	0.150
Quarter 6	533.50	447.95	85.55	0.361
Quarter 7	543.85	503.85	40.00	0.682
Quarter 8	472.03	544.47	-72.45	0.461
Sample size	250	234		
Women not living with own child(ren), including those who did not have any				
Total earnings ($)				
2-year total	5,070.23	5,290.80	-220.58	0.667
Year 1	1,607.50	2,030.02	-422.53*	0.055
Year 2	3,462.73	3,260.78	201.95	0.603
Quarter 1	191.97	285.26	-93.29**	0.045
Quarter 2	353.67	482.98	-129.31*	0.060
Quarter 3	465.73	617.07	-151.34*	0.052
Quarter 4	596.12	644.71	-48.59	0.540
Quarter 5	766.80	660.17	106.63	0.254
Quarter 6	796.34	842.71	-46.36	0.662
Quarter 7	860.12	881.52	-21.39	0.850
Quarter 8	1,039.45	876.38	163.07	0.177
Sample size	261	223		

(continued)

TABLE 5.5 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 5.6

IMPACTS ON YEAR-TWO EARNINGS, BY SELECTED CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT

		Total Earnings, Months 13-24					
Characteristic and Subgroups	Sample Size	Experimentals	Controls	Subgroup Impact	p^a	Subgroup Impact Difference[b]	p^a
Gender						$825.86*	0.051
Women	968	$3,014.53	$2,827.69	$186.84	0.521	---	---
Men	871	4,861.13	5,500.15	-639.02**	0.037	---	---
Ethnicity						---**	0.029
White, non-Hispanic	155	5,701.77	5,020.92	680.85	0.348	---	---
Black, non-Hispanic	840	3,366.72	3,788.33	-421.61	0.177	---	---
Hispanic	783	4,181.72	4,104.31	77.42	0.811	---	---
Other	61	2,735.26	5,873.74	-3,138.48***	0.007	---	---
Ethnicity, by gender						---***	0.008
Women							
White, non-Hispanic	84	4,625.78	4,447.13	178.65	0.856	---	---
Black, non-Hispanic	445	2,579.67	2,757.32	-177.64	0.679	---	---
Hispanic	413	3,196.49	2,670.38	526.11	0.236	---	---
Other	26	2,886.25	2,134.60	751.64	0.677	---	---
Men							
White, non-Hispanic	71	6,889.61	5,642.79	1,246.82	0.248	---	---
Black, non-Hispanic	395	4,166.06	4,900.26	-734.20	0.106	---	---
Hispanic	370	5,304.72	5,730.34	-425.62	0.364	---	---
Other	35	3,100.61	8,839.13	-5,738.52***	0.000	---	---
Parental status						---	0.254
Women living with own child(ren)							
No	484	3,112.84	2,814.14	298.70	0.470	---	---
Yes	484	2,685.83	2,609.55	76.28	0.853	---	---
Men who have own child(ren)							
No	765	4,928.60	5,527.54	-598.93*	0.067	---	---
Yes	106	5,435.80	6,369.78	-933.98	0.293	---	---

(continued)

TABLE 5.6 (continued)

		Total Earnings, Months 13-24					
Characteristic and Subgroups	Sample Size	Experimentals	Controls	Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
Employed within past year						-$234.26	0.581
No	870	$3,305.70	$3,633.85	-$328.16	0.285	---	---
Yes	969	4,422.13	4,516.03	-93.90	0.747	---	---
Prior employment, by gender						---	0.146
Women employed within past year							
No	547	2,590.02	2,448.71	141.31	0.715	---	---
Yes	421	3,378.85	3,119.44	259.41	0.559	---	---
Men employed within past year							
No	323	3,900.16	5,034.41	-1,134.25**	0.025	---	---
Yes	548	5,582.91	5,924.14	-341.22	0.377	---	---
Left school in grade 11 or 12						-194.85	0.651
No	1,078	3,515.58	3,800.70	-285.12	0.301	---	---
Yes	761	4,428.40	4,518.67	-90.27	0.784	---	---
Received occupational training within past year						532.95	0.343
No	1,529	3,900.76	4,010.83	-110.08	0.634	---	---
Yes	310	3,861.27	4,504.30	-643.03	0.210	---	---
Age						-175.96	0.716
16-19	1,359	3,841.64	4,092.42	-250.78	0.308	---	---
20 or 21	480	4,042.95	4,117.76	-74.82	0.857	---	---
Age, by gender						---	0.134
Women							
16-19	710	3,095.58	2,801.65	293.94	0.388	---	---
20 or 21	258	2,630.77	2,786.01	-155.24	0.782	---	---
Men							
16-19	649	4,649.11	5,506.21	-857.10**	0.016	---	---
20 or 21	222	5,572.18	5,610.03	-37.85	0.951	---	---

(continued)

TABLE 5.6 (continued)

Characteristic and Subgroups	Sample Size	Total Earnings, Months 13-24 Experimentals	Total Earnings, Months 13-24 Controls	Subgroup Impact	p^a	Subgroup Impact Difference^b	p^a
Marital status						$470.33	0.515
Ever married	174	$4,992.85	$4,771.79	$221.05	0.747	---	---
Never married	1,665	3,779.74	4,029.01	-249.27	0.261	---	---
Living in own household or with boy/girlfriend						331.27	0.543
No	1,500	3,773.51	3,909.09	-135.58	0.561	---	---
Yes	339	4,441.04	4,907.89	-466.85	0.342	---	---
Own AFDC case or receiving General Assistance						165.29	0.728
No	1,344	3,989.80	4,150.59	-160.80	0.515	---	---
Yes	495	3,630.38	3,956.47	-326.09	0.422	---	---
Own AFDC case						51.10	0.921
No	1,446	3,951.62	4,145.36	-193.74	0.416	---	---
Yes	393	3,681.78	3,926.62	-244.84	0.592	---	---
Receiving Food Stamps						528.37	0.225
No	1,143	3,979.86	3,984.51	-4.65	0.986	---	---
Yes	696	3,754.75	4,287.77	-533.01	0.120	---	---
Arrested since age 16						-319.48	0.595
No	1,567	3,928.15	4,180.31	-252.15	0.271	---	---
Yes	272	3,692.96	3,625.63	67.33	0.903	---	---

(continued)

TABLE 5.6 (continued)

Characteristic and Subgroups	Sample Size	Total Earnings, Months 13-24		Subgroup Impact	p[a]	Subgroup Impact Difference[b]	p[a]
		Experimentals	Controls				
Lived with both parents at age 14						$695.93	0.116
No	1,198	$3,773.37	$3,735.32	$38.05	0.884	---	---
Yes	641	4,111.52	4,769.40	-657.88*	0.065	---	---
Reason for leaving regular high school						---	0.456
School-related	886	4,017.62	3,917.67	99.95	0.742	---	---
Job-related	182	4,991.49	5,512.52	-521.03	0.443	---	---
Other	771	3,512.62	3,916.94	-404.32	0.212	---	---

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research assignment and, one at a time, the baseline characteristics indicated (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-subgroup impact and also to each difference between subgroup impacts. For each characteristic with more than two subgroups, an F-test was applied to the interaction between that characteristic and experimental or control status. The columns labeled "p" are the statistical significance levels of each impact and each difference in impacts or F-statistic: that is, p is the probability that sample estimates are non-zero only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic that has only two subgroups, the subgroup impact difference is the impact within the first subgroup, less the impact within the second subgroup.

(earnings) and examine how much each group was working.[18] Such an analysis shows that even though the proportion of experimentals working during the follow-up period drew even with that of controls, experimentals continued to lag behind controls in the actual time spent working (hours per week and weeks per month). Also, hourly wage information will be presented for those experimentals and controls who were working, as a way to provide some information on the types of jobs they found.

A. Impacts on the Hours and Weeks of Employment

As reported above, JOBSTART had a slightly positive impact on total two-year earnings for women living with their own children, a small negative impact for other women, and a clearly negative impact for men. This pattern carries through in hours and weeks worked, as shown in Table 5.7. On both measures, male experimentals worked less than did male controls in each year, although the difference was larger and statistically significant in the first year. For women living with their own children, experimentals were only slightly below controls (the differences were not statistically significant) in average hours worked per week and average weeks worked per month during the first year of follow-up. In the second year, experimentals moved slightly above controls on both measures, although the differences were again not statistically significant. Thus, for the entire follow-up period, experimentals were virtually the same as controls on both measures. For other women, negative impacts in the first year were larger than small positive impacts in the second, leaving experimentals in this group slightly behind on both measures for the entire two-year follow-up period.

Table 5.7 tells another interesting gender-related story. Control group men spent much more time working than did their control group counterparts among women – especially young mothers – so it is no wonder that male experimentals sacrificed more employment and earnings to take part in JOBSTART than did women. During the first year of follow-up, male controls worked an average of more than a week more per month than did controls among women living with their own children, and about half a week more than did other women in the control group. They also averaged about nine hours per week more than did women living with their own children, and five hours more than other women. These gender differences in

---

[18] This effort to understand the pattern of impacts is also the topic of Chapter 6, which discusses results by site.

2 8

TABLE 5.7

IMPACTS ON WEEKS EMPLOYED PER MONTH AND HOURS WORKED PER WEEK THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Average number of weeks employed per month during				
Months 1-24	1.74	1.98	-0.24***	0.006
Months 1-12	1.32	1.71	-0.40***	0.000
Months 13-24	2.17	2.26	-0.09	0.432
Average number of hours worked per week during				
Months 1-24	15.02	17.36	-2.34***	0.005
Months 1-12	10.74	14.37	-3.64***	0.000
Months 13-24	19.30	20.34	-1.04	0.329
Sample size	438	433		
Women living with own child(ren)				
Average number of weeks employed per month during				
Months 1-24	0.78	0.78	0.00	0.971
Months 1-12	0.61	0.66	-0.05	0.577
Months 13-24	0.96	0.90	0.06	0.621
Average number of hours worked per week during				
Months 1-24	6.08	6.32	-0.24	0.764
Months 1-12	4.56	5.15	-0.59	0.483
Months 13-24	7.60	7.50	0.10	0.928
Sample size	250	234		
Women not living with own child(ren), including those who did not have any				
Average number of weeks employed per month during				
Months 1-24	1.28	1.35	-0.07	0.543
Months 1-12	0.97	1.20	-0.23*	0.055
Months 13-24	1.59	1.50	0.09	0.534
Average number of hours worked per week during				
Months 1-24	10.20	11.00	-0.80	0.401
Months 1-12	7.12	9.40	-2.28**	0.017
Months 13-24	13.27	12.59	0.68	0.610
Sample size	261	223		

(continued)

TABLE 5.7 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

controls' hours and weeks worked were much greater than the experimental-control differences in hours within each subgroup.

B. Differences in Wage Rates

Earnings are the product of time spent working (discussed above) and wages earned per hour. Departing from the analysis so far, the discussion in this section excludes some experimentals and controls (those who did not work during the periods indicated) because wage rates were not observed for nonworkers. Thus, the experimental-control differences discussed here are not impacts, but they do help us understand the experience underlying the impact findings.[19]

Overall, both experimentals and controls showed a gradual growth over time, although the trends are not always clear. Wages for working experimentals began at about $4.49 per hour and grew to about $5.14 per hour; for working controls, wages began at approximately $4.43 per hour and grew to about $5.26 per hour. Although only a 34-cent advantage for working controls over working experimentals in month 10 was statistically significant, working controls earned more per hour than working experimentals in most months.

Splitting the sample by gender reveals that positive wage differences for working women almost counterbalance negative wage differences for working men each month. For example, in month 24, among working men, experimentals earned $5.03 and controls earned $5.39 per hour, for a difference of minus 36 cents, while among women, experimentals earned $5.32 and controls earned $5.01, for a difference of plus 32 cents. Among working women, experimentals overtook controls in hourly wages at about the same time they overtook them in employment and earnings. Among working men, experimentals continue to lag behind controls on this measure just as they do in earnings.

V. JOBSTART's Impacts on Positive Activity

A second step in understanding the short-term employment and earnings impact findings is to examine whether access to JOBSTART led more experimentals than controls to spend their time in "positive activity," defined as either work or further education and training. Experimentals might have been working less than controls because experimentals were continuing to invest time in enhancing their skills.

---

[19] This is because of the likelihood of selection bias in the "choice" of those experimentals and controls who worked. While random assignment created experimental and control groups with similar characteristics at random assignment – except that experimentals had access to JOBSTART – it is not likely that working experimentals and working controls were similar except for access to JOBSTART.

This does seem to have been the case for much of the follow-up period. Figures 5.2 through 5.4 present impacts on positive activity for men, women living with their own children, and other women. For men, experimentals exceeded controls by a statistically significant amount for the first 10 months of follow-up and months 15 and 16. For women living with their own children, experimentals exceeded controls for virtually all the first 18 months. For other women, experimentals exceeded controls for much of the first 10 months and then sporadically thereafter.

The basic conclusion is that one important reason for lower earnings for experimentals and controls was sustained greater investment in education and training by the JOBSTART youths. This could signal an improved earnings situation once longer follow-up is available. However, during the 24 months of follow-up, no strong turnaround occurred.[20]

VI. JOBSTART's Impacts on Other Outcomes

If JOBSTART is successful, other aspects of the JOBSTART youths' lives may also change. Key outcomes of interest include receipt of public benefits, criminal activity, and marital and parental status. Even at this early stage of follow-up, when the employment impact story has not yet been told, it is useful to summarize briefly early findings on these other aspects of the young people's lives.

During the first 24 months of follow-up, JOBSTART had virtually no significant pattern of impacts on the receipt of a long list of public benefits, including AFDC, Food Stamps, and unemployment insurance benefits. This general conclusion is true for the full sample and both women and men. The two exceptions are: (1) In some months, experimental men were more likely to receive General Assistance than were control men (although the payments were never higher by a statistically significant amount); and (2) in some months, experimental women were

---

[20] For example, in the final quarter of follow-up, when employment rates and rates of productive activity were about equal, the average earnings of controls remained slightly ($61) above those of experimentals for the full impact sample.

FIGURE 5.2

MONTHLY RATES OF POSITIVE ACTIVITY FOR MEN, BY RESEARCH STATUS

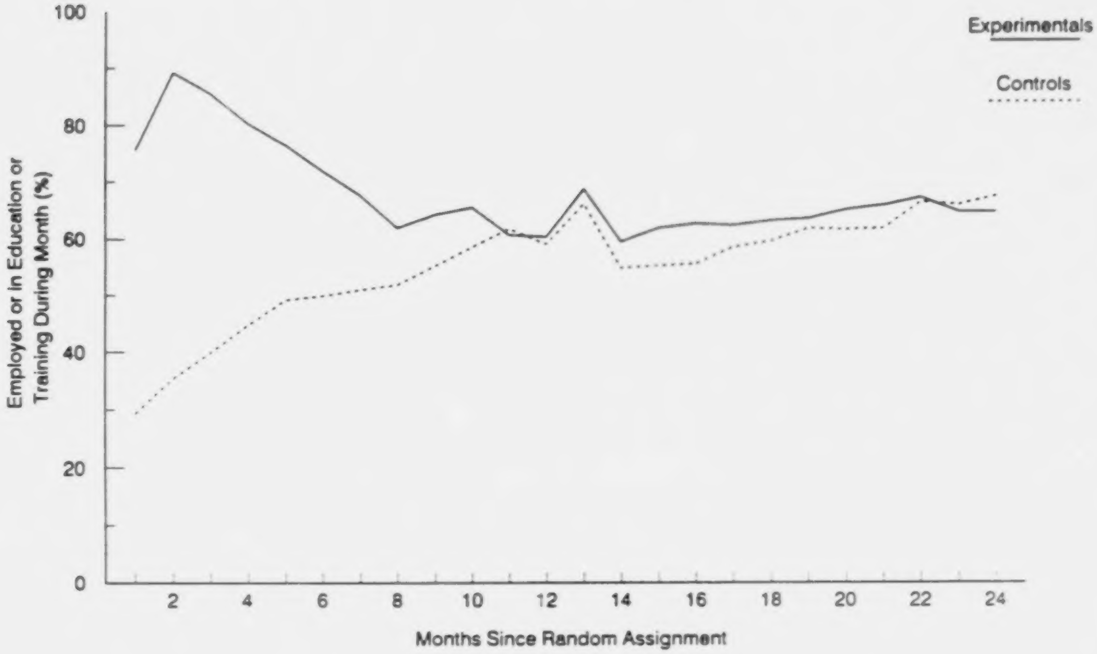


FIGURE 5.3

MONTHLY RATES OF POSITIVE ACTIVITY FOR WOMEN LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS

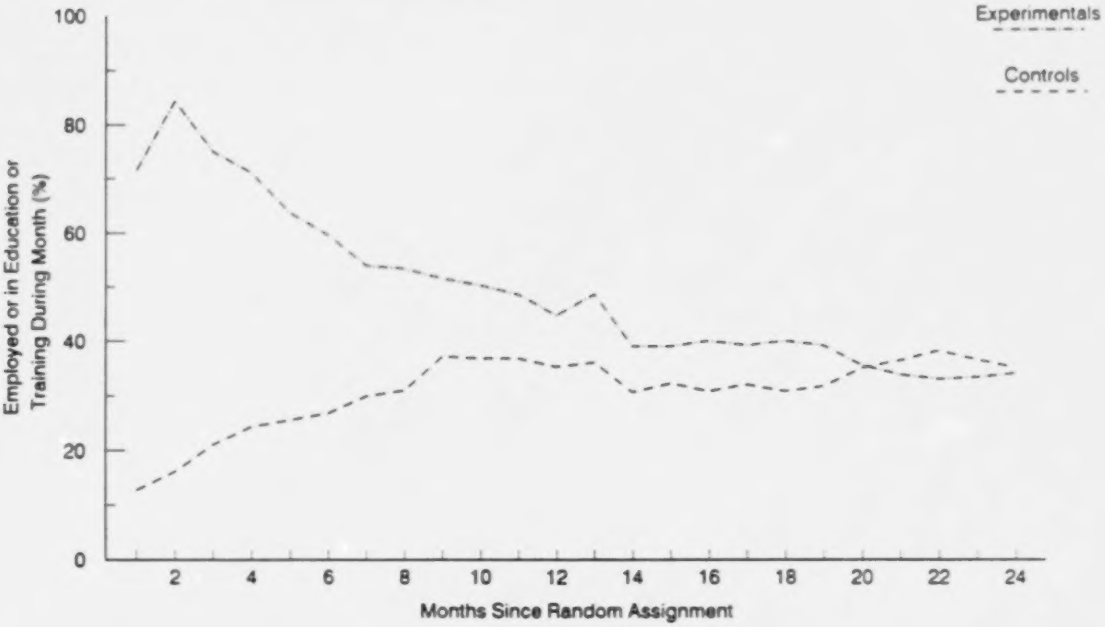
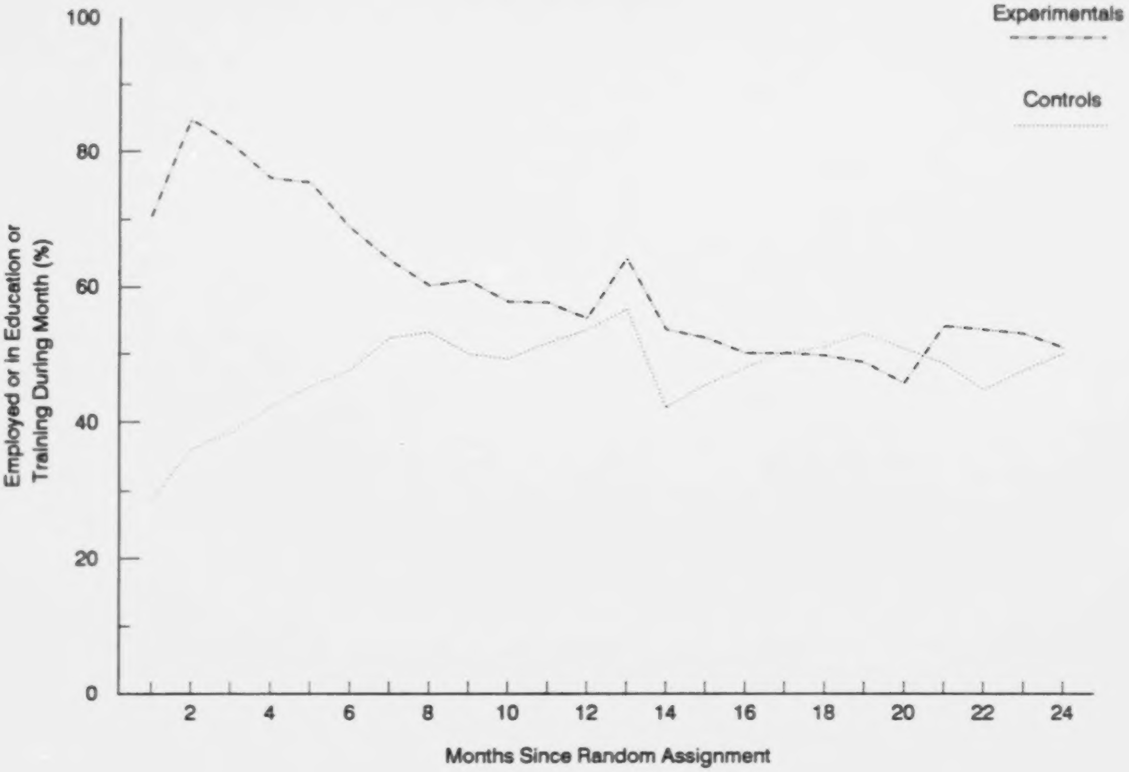


FIGURE 5.4

MONTHLY RATES OF POSITIVE ACTIVITY FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN), BY RESEARCH STATUS*



*Includes women who did not have children.

SOURCE FOR FIGURES 5.2-5.4: Appendix Table F.6.

NOTES FOR FIGURES 5.2-5.4: Calculations for these figures used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Positive activity" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities, as well as employment.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

slightly less likely to receive Supplemental Security Income (SSI) and received less in payments than did control women.

By this early time in post-program follow-up, JOBSTART has not had any statistically significant impact on criminal behavior, as evidenced by arrests during months one through 24 of follow-up (Table 5.8). For men and mothers, experimentals had a slightly lower rate of arrest than did controls, while for other women, experimentals were slightly above controls. However, none of the differences were large.[21] Impacts were also favorable for many other subgroups, but virtually none was large enough for statistical significance.[22]

This picture differs from that produced by the previous study of the residential Job Corps program, but there is a clear reason to expect much less of an early impact on criminal behavior for JOBSTART. In the residential Job Corps program, young people moved from their community to a special center, which provided the education and training services. Often, these centers are in isolated areas or in communities without large gang populations or heavy involvement of youths in the drug trade. Much of the Job Corps' impact on criminal behavior came during the in-program period because of this "isolation" effect. In JOBSTART, no such change occurred in the young people's lives: They continued to live in their own neighborhoods and spent time outside the program with their existing circle of friends.

A final dimension of program effects to consider is childbearing. According to Table 5.9, among those women who were custodial parents at random assignment, assignment to the JOBSTART experimental group was associated with a statistically significant increase in childbirth by month 24; the increase was 7.3 percentage points. Among other women, the vast majority of whom were childless at random assignment, JOBSTART lowered the rate of childbirth by 2.9 percentage points, although this difference was not statistically significant. These findings are important because they show the extent to which childbearing may be inhibiting greater labor market impacts among JOBSTART women.

---

[21] Among men, 30.2 percent of experimentals and 33.4 percent of controls were arrested during the follow-up period. Among women, 7.6 percent of experimentals and 8.0 percent of controls were arrested.

[22] The only exceptions were the impact on arrests for white, non-Hispanic youths (especially men) and on youths reading below the sixth-grade level at entry into the program.

TABLE 5.8

IMPACTS ON CRIMINAL ARRESTS THROUGH MONTH 24, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Ever arrested, months 1-24 (%)	30.2	33.4	-3.2	0.290
Sample size	438	433		
Women living with own child(ren)				
Ever arrested, months 1-24 (%)	6.8	9.4	-2.6	0.306
Sample size	250	234		
Women not living with own child(ren), including those who did not have any				
Ever arrested, months 1-24 (%)	8.0	6.8	1.1	0.645
Sample size	261	223		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 5.9

IMPACTS ON PREGNANCY AND CHILDBIRTH THROUGH MONTH 24, BY PARENTAL STATUS AT THE TIME OF RANDOM ASSIGNMENT

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Women living with own child(ren)				
Ever pregnant, months 1-24 (%)	59.1	53.1	6.0	0.191
Ever gave birth, months 1-24 (%)	32.7	25.4	7.3*	0.087
Sample size	250	234		
Women not living with own child(ren), including those who did not have any				
Ever pregnant, months 1-24 (%)	45.6	47.5	-1.9	0.678
Ever gave birth, months 1-24 (%)	18.5	21.4	-2.9	0.443
Sample size	261	223		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

CHAPTER 6

SITE VARIATION IN IMPACTS

Chapters 4 and 5 provided estimates of JOBSTART's impacts on educational attainment, employment, and earnings for the full sample and for key demographic subgroups. This chapter addresses another important issue: How were impacts affected by program characteristics? The most direct way to explore this issue within the JOBSTART Demonstration is to examine differences in individual *site* impacts, since there were some programmatic differences among the 13 sites and the programs operated in a variety of settings. For reasons soon to be discussed, this analysis of impacts across sites, of necessity, is less certain than that reported in the previous two chapters.

While the limited but still relatively large number of sites in JOBSTART is clearly an advantage in this effort, it does not assure success in "teasing out" how program characteristics affected impacts. As with virtually all multi-site demonstrations, the JOBSTART Demonstration was not designed to address, with the rigor of an experiment, the question of how differences in program structure influence impacts. The major goal of the demonstration was to ascertain whether the model, as implemented in a diverse sample of sites, would lead to improved employment and earnings. For this purpose, youths in each site were randomly assigned to the experimental and control groups.

To answer, with similar rigor, the question of how program characteristics affect impacts, youths *in individual sites* would have had to be randomly assigned to one of the several types of programs of interest (for example, those emphasizing education versus those emphasizing training), and individual sites would have had to offer more than one type of program. This kind of study – known as a "differential impact" research design – would be needed to separate out clearly the influence of program type from other site variations such as local labor market conditions and differences in the kinds of youths drawn to the various sites.[1] This approach was not followed in light of the main goal of the study: to use the combined cross-site sample to answer impact questions about the full sample and subgroups.

---

[1] For example, as discussed in Chapter 2, sites in the JOBSTART Demonstration did vary in the proportions of their youths who had prior work experience and welfare receipt, and in the proportion with children and from the black and Hispanic ethnic groups.

Nevertheless, the JOBSTART Demonstration did provide an opportunity to learn more about the link between program characteristics and impacts. For example, if the respective categories of site programs had shown clear differences in impacts, certain lessons might have been drawn (including the *lack* of influence of other factors). As the findings in this chapter show, however, no clear pattern emerged: That is, no single category of sites did appreciably better than another in terms of two-year impacts. Instead, both positive and negative impacts appeared *within each category*, implying that JOBSTART can produce positive (or negative) impacts in a variety of settings and with a variety of program structures. No single approach either assures positive impacts or is necessary for them.

Before turning to the analysis per se, some additional preliminaries may be useful. Much of the previous analysis in this report divided sites into three groups – concurrent, sequential/in-house, and sequential/brokered – because these categories helped explain differences in program implementation and participation among the sites.[2] This chapter preserves these site groupings, since the implementation research led to a hypothesis that these differences in program structure could influence impacts. But it is important to keep in mind that these categories are not a neat division of the sites into homogeneous groups. One key difference within the concurrent category was discussed in Chapter 1 and elsewhere in the report: Two sites (CET/San Jose and Chicago Commons) integrated education into the training curriculum to a much greater extent than did the other concurrent sites. Other characteristics that varied within categories included the extent to which applicants were initially screened, the quality of sites' implementation of the four JOBSTART components, and labor market conditions. Thus, disentangling the independent effect of any single program feature on impacts across sites can be very difficult.

One final point should be made: The impacts on employment and earnings at the individual site level (even when noticeably different from zero) were usually not statistically significant.[3] There are three reasons for this: (1) Most of the reported employment and earnings impacts were not large; (2) a site's program did not affect the behavior of all youths in the same way, and controls' earnings also varied (in technical terms, this suggests a large variance in the outcomes); and (3) the sample size in each of the sites was relatively small. Furthermore, the *differences* in the individual site impacts for employment and earnings were also not statistically significant, suggesting again that lessons on program structure must be tentative.[4]

---

[2] For example, the sequential/brokered sites all had difficulty getting young people to make the transition to training, and the concurrent sites all tended to emphasize training (as opposed to education) more than the other sites.

[3] This was not true for site-level impacts on educational attainment, which in many sites were large and statistically significant.

To summarize the implications of these fairly technical issues: Only if there was a clear *pattern* of impacts among types of sites should lessons be drawn from this analysis about how one particular program feature affected impacts. Furthermore, in general, conclusions should not be drawn from the impacts at any single site.

Despite these difficulties of analysis and interpretation, this chapter does try to draw some conclusions about the link between program features and impacts. It moves into this type of analysis because of the strong interest in identifying more effective ways to structure education and training programs, an interest intensified by the recent publication of the first impact report from the Minority Female Single Parent (MFSP) Demonstration, sponsored by the Rockefeller Foundation.[5] That study analyzed program impacts in four sites offering very different types of education and training for low-income, minority mothers who averaged 28 years of age. One site, the Center for Employment Training (CET) in the San Francisco Bay area, offered an integrated program of training and education, with little initial screening of applicants and intensive job placement efforts. This site showed strong employment and earnings impacts in the fourth quarter of follow-up, while in the remaining three sites – each with an emphasis on basic skills instruction, which was to be followed by various types of occupational training – experimentals and controls had approximately equal employment and earnings. Some have attributed this difference in impacts between CET and the other sites to specific features of its program, especially its integration of education and training. CET/San Jose was a part of both the JOBSTART and MFSP demonstrations, although in JOBSTART, the CET/San Jose sample was made up primarily of men and women without children, since most young mothers were part of the MFSP Demonstration.

This chapter proceeds with a framework for analyzing site-level impacts. It then discusses site-level impacts for the full impact sample and for the three key subgroups of men, women living with their own children, and other women. It closes with a summary of the findings and directions for future research.

---

[4] Statistical significance in this context is a test of whether the extent of variation in impacts *across* sites was so systematic that it was unlikely to have arisen by chance.

[5] See Gordon and Burghardt, 1990.

I. A Framework for Analyzing Site Variation in Impacts

This section discusses how JOBSTART sites varied in ways that could influence program impacts. It then presents two alternative types of site impact estimates used in this chapter.

A. Factors Leading to Site Variation in Impacts

The differences in program structure among the sites, discussed earlier in this report, were among the most obvious possible influences on site impacts. However, these program factors were entangled and confounded across JOBSTART sites with several other sources of variation in impacts including the target group recruited at various sites and their relative interest in education per se as opposed to training, the extent of screening by site operators, the point at which random assignment was placed for the evaluation, the availability of local alternative services for members of the control group, and environmental factors such as the wage structure and tightness of the labor market.

1. Program structure. As detailed in Chapter 3, dimensions of program structure that are of special interest include concurrent versus sequential education and training classes, the extent of integration of education and training, months of program activities offered and delivered, brokering of services among multiple vendors versus in-house provision of all services, the relative emphasis on education as opposed to occupational skills training, the strength of job placement efforts, and the intensity of support services. Table 6.1 combines selected findings from earlier tables to highlight key aspects of the sites. Sites are grouped under the headings "concurrent," "sequential/in-house," and "sequential/brokered."

"Length of stay" is an important dimension of site variation when examining follow-up in a short period such as two years. If programs were able to provide equal hours of instruction, in sites in which experimentals participated for shorter periods of time, there will have been more post-program time during which experimental employment outcomes could have overtaken control outcomes at the two-year point. CET/San Jose and Chicago Commons are notable on this score, with average lengths of stay less than two-thirds of the full sample average, even though total hours were higher than average at Chicago and 88 percent of the average at CET/San Jose. The "average hours per month" column is a measure of the extent to which

TABLE 6.1

SUMMARY OF JOBSTART IMPLEMENTATION, BY SITE

Site	Total Hours	Length of Stay (Months)	Average Hours per Month	Hours of Education	Hours of Training	Level of Initial Screening	Rating of Job Placement	Rating of Support Services	Overall Rating of Implementation
Concurrent	405	6.4	60	104	291	---	---	---	---
Atlanta Job Corps	296	5.5	50	95	150	High	Low	High	Low
CET/San Jose[a]	366	4.4	74	29[b]	337	Low	High	Medium	High
Chicago Commons[a]	445	4.5	83	72[b]	373	High	High	Medium	High
Connelley (Pittsburgh)	579	10.1	54	105	473	Medium	Medium	High	High
East LA Skills Center	345	5.4	55	73	272	Medium	Low	Medium	Medium
EGOS (Denver)	274	7.1	33	126	148	Low	Low	Medium	Low
Phoenix Job Corps	441	6.5	60	163	218	High	High	High	High
SER/Corpus Christi	418	5.2	76	124	294	Medium	Low	High	Medium
Sequential/in-house	536	7.1	66	152	287	---	---	---	---
El Centro (Dallas)	401	5.8	60	142	179	Medium	Medium	Medium	High
LA Job Corps	631	8.1	71	159	362	High	Medium	High	High
Sequential/brokered	316	7.8	37	178	78	---	---	---	---
Allentown (Buffalo)	440	10.7	39	244	113	Medium	Low	High	Medium
BSA (NYC)	280	5.5	45	145	66	Low	Low	Medium	Low
CREC (Hartford)	166	6.3	23	119	36	Low	Low	Medium	Low
All sites	415	6.8	57	128	249	---	---	---	---

SOURCES: MDRC calculations from JOBSTART enrollment form, MIS, and survey data (participation figures); MDRC operations staff (implementation ratings).

NOTES: [a] In this site, education and training were more integrated than in other sites, and staff strongly emphasized training over passing the GED examination.

[b] In this site, some education hours are included in the training component hours.

education, training, and other hours were concentrated or spread out over time. In general, the shorter the length of stay, the higher were the hours per month. CET/San Jose and Chicago Commons, the two sites with the shortest length of stay, had high average hours per month because they squeezed just about as much program activity as the average site had into shorter periods of time.

CET/San Jose and Chicago Commons had two things in common that help explain their relatively short length of stay. First, the programs were concurrent, offering education and training at the same time rather than one after the other. Second, there was less emphasis on education leading to a GED than on skills training in these integrated or partly integrated programs. Thus, youths in these programs typically stayed fewer months than those in sequential programs and in other concurrent programs that emphasized education more. For these reasons, one might hypothesize that second-year earnings impacts in these two sites might be greater than those in sites that provided the same level of services over a longer period of time. Opportunity costs (forgone employment and earnings) were smaller, and experimentals in the two sites had more time to catch up with and overtake controls in employment rates and earnings by month 24. One might also hypothesize that sites providing more services over a longer period could have greater *longer-term* impacts. This issue will be clarified when the four-year follow-up results are known.

The "hours of education" column shows much variation, but there was a tendency for the sequential – and especially sequential/brokered – sites to emphasize education more than the concurrent sites. Four of the eight concurrent sites averaged more than 50 hours less than the average for all sites, and only one of them exceeded the all-sites average. It stands to reason that concurrent sites would have delivered fewer hours of education than sequential sites because concurrent programs had to squeeze more activities into the school day. Variation in the proportion of youths getting any education was not a major factor in site variation in hours: Table 3.9 showed that, except at CET/San Jose (which did not offer separate education classes), this proportion varied in a narrow range around the full sample average of 86 percent.

The "hours of training" column shows more site variation than the previous columns. As already indicated, the dramatic difference between the sequential/brokered sites and the others on this score probably reflected both the systemic and administrative problems the former encountered in linking education and training and differences in the interests and expectations of recruits. Table 3.10 showed that only one quarter of experimentals at sequential/brokered sites got any occupational skills training at all. One surprise here is that the two sequential/in-house programs managed to deliver hours of training comparable to the hours in concurrent programs. This achievement is notable, since only half the experimentals in sequential/in-house sites stayed long enough to receive any training, while more than 80 percent of experimentals in concurrent sites received training.

The "job placement" column repeats the subjective ranking given in Table 3.4 for the strength of job placement assistance efforts in a site. CET/San Jose, Chicago Commons, and the Phoenix Job Corps were considered especially strong on this score; Connelley in Pittsburgh, El Centro in Dallas, and the Los Angeles Job Corps were considered middling; and all the other sites were considered weak. Looking at this column by itself would lead one to predict high labor market impacts early on at CET/San Jose, Chicago Commons, and the Phoenix Job Corps. However, the effectiveness of various types of job placement assistance could be affected by labor market conditions.

The "support services" column is taken from Table 3.4 and assesses the strength of the implementation of the fourth component of JOBSTART, support services. Along with the three Job Corps nonresidential programs, Connelley in Pittsburgh, SER/Corpus Christi, and Allentown in Buffalo stood out from the other sites in the strength of their efforts to deal with problems that stood in the way of program attendance and completion.

To sum up this table, CET/San Jose, Chicago Commons, Connelley in Pittsburgh, El Centro in Dallas, and two of the three Job Corps programs seem to have implemented JOBSTART's four components more fully than did the other sites. Thus, these sites appear to have set the stage for program impacts stronger than those in the other sites. However, the relatively strong treatments, short lengths of stay, and resulting intensity (hours per month) at CET/San Jose and Chicago Commons could have given these two sites an edge over the others in a *short* follow-up period.

2. Recruitment. While all JOBSTART sites had a common target group for the demonstration (disadvantaged young dropouts reading below the eighth-grade level), there were variations in site recruiting emphases and in participant characteristics. These site variations arose because different types of youths applied and because intake practices were not uniform.

Some characteristics of youths vary in easily measured ways. For example, as reported in Chapter 2, sites varied greatly in the proportion of the sample that was made up of youths who had recent work experience or who were young mothers. Factors such as these were measured at random assignment and can, to a considerable extent, be adjusted for statistically in comparing program impacts across sites. One type of impact estimate reported later in this chapter does include such statistical adjustments in an effort to control for this one source of variation among sites and to move closer to isolating variation in impacts caused by program structure.[6]

Other factors are much harder to observe and were, therefore, not measured at entry into the sample; these cannot be included in statistical adjustments. One especially important factor is the youths' goals in participating in the program and their resulting interest in the various JOBSTART components. Although the youths' goals and interests were not observed directly in any quantifiable way, the reputation of the site in the community probably had a strong influence on who applied for the program. Those sites with a history of providing basic skills instruction and not occupational training (BSA in New York City, Allentown in Buffalo, and CREC in Hartford) naturally would seem to have attracted youths who were more interested in GED attainment than in immediate acquisition of marketable occupational skills, while those sites emphasizing occupational skills training (CET/San Jose and Chicago Commons) seem to have attracted those who were more interested in job skills and immediate employment than in education.

Available information does indirectly support this generalization. Table 3.10 showed that JOBSTART hours of education tended to be high and that hours of occupational skills training were lowest in sequential/brokered sites, although the latter clearly resulted partly from the administrative problems those sites encountered in linking education and training. Taken together, these findings are consistent with the idea that recruits in the sequential/ brokered sites were less interested in occupational training per se than in education leading to a GED. In other words, these sites may have delivered fewer hours of skills training partly because their recruits demanded fewer hours.

3. **Screening**. Some sites – such as CET/San Jose, EGOS in Denver, BSA in New

---

[6] Although these impacts include linear statistical adjustments for these characteristics, not all relevant characteristics were measured; there might be differential errors of measurement of characteristics; true relationships between impacts and characteristics may be nonlinear; or impacts of sites with very unusual sample characteristics might be difficult to adjust properly with any statistical model. Thus, adjusted site outcomes and impacts must be viewed with a special caution not applicable to estimated subgroup impacts.

York City, and CREC in Hartford – did not screen applicants much before they entered the sample, while others (such as Chicago Commons and the Job Corps sites) carried out extensive screening.[7] When a great deal of screening takes place, it is reasonable to assume that those randomly assigned (including those assigned to control group status) will have high levels of perseverance, motivation to go into a program, motivation to work, and other important characteristics hard to measure directly. Thus, all else being equal, in sites that screened heavily, control group outcomes should have constituted tougher benchmarks for experimentals to surpass than in sites that carried out minimal screening. However, minimal screening may yield groups of experimentals who are harder to keep in the program and harder to place at completion.

4. **Point of random assignment.** On top of the normal steps and resulting screening in recruitment and intake, the research design introduced random assignment, which could occur at slightly different points relative to initial contact with a potential participant and actual start-up of services. As mentioned in Chapter 2, putting random assignment early in the program flow (as at CET/San Jose) tended to include in the sample individuals who might drop out of the later steps of intake, while putting random assignment relatively late (as at Allentown in Buffalo) meant that only those making it to that stage of intake were in the research sample. Late random assignment tended to raise the proportion of the experimental group participating in JOBSTART, because it meant that those randomly assigned were interested enough in the program to persevere through the steps of intake and because it reduced the wait between random assignment and the start of services. It also tended to raise the proportion of controls who were served in alternative programs, because those randomly assigned to the control group were also quite motivated to receive services.[8]

5. **Service availability.** More programs tend to be available in large cities, such as

---

[7] Chapter 3 pointed out the severe recruitment pressures at CET/San Jose during intake for JOBSTART. CET/San Jose may have been able to adopt this "no screening" policy because of the characteristics of disadvantaged youths in its service area. Even with open admissions, a higher percentage of the CET/San Jose youths had recent work experience than did the full sample. This probably occurred because CET/San Jose drew largely Hispanic youths, a group that, as mentioned in Chapter 1, typically has a higher labor force participation rate than do other minority school dropouts.

[8] The correlation coefficient of sites' percentage of experimentals and controls participating in education and training is +.37. The point of random assignment thus had implications for the proportion of experimentals and controls receiving services, but both groups were affected similarly, so there was no consistent effect on the *difference* in the proportion of experimentals and controls receiving some type of education and training.

New York, Chicago, and Los Angeles, than in smaller cities such as Corpus Christi or Hartford. Since the impacts reported here are the impacts of JOBSTART compared to the alternative services in which the controls participated (rather than the impacts of JOBSTART services versus no services), variation in the controls' benchmark levels of services will influence impacts. As Table 6.2 shows, the level of control services and the resulting difference between experimentals and controls in service receipt varied greatly by site.[9] For the most part, service receipt differences were smaller in sequential/brokered sites than in other sites. The largest service differentials were found at El Centro in Dallas and SER/Corpus Christi.[10]

6. Wage structure and labor market tightness. In some communities, jobs tend to be plentiful but low-paying; in others, they tend to be scarcer but higher-paying; and in still others, they are both scarce and low-paying. This could be the result of long-term differences in the industrial base of the communities or of how they are affected by business cycles. These labor market conditions could affect both the control group's level of employment and earnings and the availability of jobs that reward the experimental group's increased educational attainment and occupational training.

B. Two Alternative Types of Site Impact Estimates

In this chapter, impacts are compared across sites to help explain how differences in program operation affected program impacts. As just discussed, however, sites differed in many ways, making it very difficult to isolate the influence of program features. The most straightforward approach is to calculate separate experimental-control comparisons for each site, in effect creating 13 separate samples (or data files) and calculating impacts for each one. In doing this type of site impact analysis, the only adjustment made is to take account of differences in the observed pre-random assignment characteristics of the experimental and control groups, which may occur when sample sizes are relatively small.

It is possible to move beyond this "split-file" analysis because one type of site variation

---

[9] This table is taken from part of Table 4.1. Differences in service receipt across sites were statistically significant. Alternative estimates of service receipt, which result from two-way analysis of the effect of site and experimental or control status, are not materially different from those in Table 6.2 (see Appendix Table G.1).

[10] As discussed in Chapters 2 and 3, at CET/San Jose some early activities were not counted in the JOBSTART data system; thus, the actual participation rate for experimentals was somewhat higher than the data suggest. Nevertheless, the reported experimental-control difference in service receipt for CET/San Jose is higher than average.

TABLE 6.2

EXPERIMENTAL-CONTROL DIFFERENCE IN SERVICE RECEIPT, BY SITE

		Ever Received Any Education or Training, Months 1-24			
Site	Sample Size	Experimentals	Controls	Difference	p[a]
Concurrent					
Atlanta Job Corps	61	92.7%	47.9%	44.8***	0.001
CET/San Jose	152	73.1	21.0	52.0***	0.000
Chicago Commons	74	93.2	60.9	32.3***	0.004
Connelley (Pittsburgh)	184	99.8	45.8	54.0***	0.000
East LA Skills Center	100	92.2	59.1	33.2***	0.000
EGOS (Denver)	183	94.2	51.6	42.6***	0.000
Phoenix Job Corps	130	92.5	36.6	55.9***	0.000
SER/Corpus Christi	236	98.8	34.6	64.2***	0.000
Sequential/in-house					
El Centro (Dallas)	155	99.3	27.8	71.6***	0.000
LA Job Corps	218	87.1	45.5	41.6***	0.000
Sequential/brokered					
Allentown (Buffalo)	140	97.8	70.6	27.2***	0.000
BSA (NYC)	119	88.3	49.3	39.1***	0.000
CREC (Hartford)	87	93.5	47.4	46.1***	0.000
All sites	1,839	92.7	44.2	48.4***	0.000

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for 19 kinds of difference in characteristics before random assignment; "all sites" outcomes are from a linear analysis of covariance procedure for the full sample controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

– the observed characteristics of the youths in the sample – can be adjusted for statistically by relying on the fact that youths with particular characteristics are rarely concentrated in just one site. This type of adjustment takes account of the program impacts for each type of youth in the full sample (for example, the impacts for blacks, those with prior work experience, or those receiving public assistance) and adjusts individual site impacts to reflect the fact that some sites served many youths who (in the full sample) tended to have lower-than-average impacts, while other sites served more than the usual percentage of youths who had higher-than-average impacts.[11]

The two approaches answer different questions, and each has strengths and weaknesses. The split-file approach is most valuable as a description of what actually occurred in each site: That is, it presents the best estimates for the impacts of the site's program as it actually operated for the people recruited and randomly assigned to the experimental group. This is the approach taken, for example, in the recent report on the MFSP Demonstration; the findings for each of the four sites were calculated separately.[12] Its chief drawback is that the differences in outcomes for experimentals and controls within each site are not fully comparable across sites because the characteristics of the two groups differ from site to site. If, for example, youths who were employed in the year prior to random assignment tended to have lower employment and earnings impacts, sites with a sample made up of youths with higher-than-average prior-year employment will have impacts lower than they would have been had those sites served a more disadvantaged mix of recruits. Furthermore, the individual site files may have relatively small sample sizes, limiting the precision of impact estimates.

The alternative of adjusting for measured site differences in pre-random assignment characteristics is most valuable in trying to isolate the effects of location per se. Sites with populations different from the overall average are not penalized or rewarded relative to other sites, as they are in the split-file approach. To stay with the same example: The estimated impacts for a site with a higher-than-average proportion of youths with prior employment would be larger using this adjustment than with the split-file approach. However, in order to make the adjustment for site differences in the mix of youths' pre-random assignment characteristics, it is necessary to assume that sites could have served – and gotten "average" impacts for – populations they actually did not serve. For example, sites set up to serve a particular type of person (for example, young men) might not be able to serve other young people. Thus, they might not attain the impact observed in the full sample for these other groups.[13] Each way of looking at site impacts answers a particular question and has its own advantages and disadvantages, so both are presented in the following section.

---

[11] For example, Chapter 5 discussed how impacts were more favorable for women living with their own children than for other women or for men. The adjustment would take account of the fact that the sample in some sites included more than the average proportion of women living with their own children (thereby raising observed site impacts) and some served more than the average proportion of men (thereby lowering observed site impacts). The adjustment was designed to remove the difference in impacts arising from these differences in each site's sample. The statistical adjustment assumes a linear relationship between each characteristic and outcomes and is the same as the method underlying Tables 4.4, 4.7, 5.3, and 5.6, which presented impacts for designated subgroups controlling for differences between the subgroups other than the characteristic used to define them. It used linear adjustments of outcomes, with up to 19 kinds of differences in youths' characteristics entered as covariates. See Ostle, 1975.

[12] See Gordon and Burghardt, 1990.

For both approaches, the impacts presented are per experimental. Appendix B, Section III, discusses how impacts per experimental can be converted to impacts per participant if certain assumptions are valid. This conversion was not done here – despite the fact that participation rates for experimentals do vary across the sites – because participation rates for controls also vary, and are somewhat correlated with the experimentals' rates: For example, sites with a high participation rate for experimentals tend to have a high rate for controls. Consequently, comparisons of the *difference* in participation *between* experimentals and controls across sites show a different pattern of variation than comparisons of the participation rate for either group individually: For instance, sites with a lower-than-average participation rate for experimentals do not necessarily have a lower-than-average difference in participation between experimentals and controls. Since the impact analysis presented here compares experimental and control outcomes and shows the impact of the increment of services received by experimentals above the services received by controls, adjusting site-specific impacts to account for cross-site variation in experimentals' participation would confuse rather than clarify comparisons across the sites.[14]

---

[13] The adjusted impacts approach uses the entire sample, lessening the problems of sample size in the analysis. However, it introduces a further complication: The adjustments implicitly assume that youths' characteristics affect outcomes in the same way in each site. More technically, the assumption is that the coefficients estimated for the regression used in making adjustment for differences in characteristics (both between experimentals and controls and across sites) are the same across all sites. The split-file approach estimates separate regressions for each site to adjust for differences in sample characteristics between experimentals and controls. Therefore, the shift from split-file impacts to those adjusted for site differences in pre-random assignment characteristics involves two types of changes: changing to the standard full sample regression coefficients and adjusting for differences across sites in sample characteristics. Consequently, it is often not possible to give a simple, intuitive explanation of why the shift from split-file to adjusted impacts caused the observed change in impacts.

II. Full Sample Impacts, by Site

The outline of the site story is the same under both types of site impact approaches, although the adjustment does make a noticeable difference in the impacts of a few sites that served a group of young people that differed markedly from the sample as a whole and/or had smaller-than-average sample sizes. Table 6.3 presents the split-file impacts, that is, those with no adjustments for differences across sites in the youths served. Table 6.4 presents impacts with that type of adjustment. Both tables include three impacts: The left section presents experimental-control differences in educational attainment; the middle section, differences in employment rate during the second year of follow-up; and the right section, differences in earnings during the second year of follow-up.

As anticipated, few of the individual site impacts were statistically significant, so lessons would come from patterns of impacts across sites. In both tables, in each site grouping, there are one or more sites with a positive impact and one or more with a negative or very small impact for each of the three outcomes: high school completion and GED receipt, and employment and earnings in the second year. As the tables show, there was variation in impacts *within* each group of sites as well as *between* groups of sites. For educational attainment, the differences in site impacts were statistically significant.[15] However, the *differences* in the individual site impacts on employment and education were not statistically significant, suggesting caution in drawing conclusions from any observed differences in impacts.

High school diploma and GED receipt impacts were mainly positive (and, in four sites, statistically significant) but fell negative at Chicago Commons and the East Los Angeles Skills

---

[14] It is possible to adjust impacts to take account of participation in services by both experimentals and controls, but this involves making untestable assumptions about the impacts of the services received by controls. See Auspos, Cave, and Long, 1988, pp. 198-199.

[15] Table 6.4 includes a test of the statistical significance of the variation in individual site impacts. The "p-value" of the "F-statistic" at the foot of each column in Table 6.4 is the probability that site variation in that impact could be due to chance alone. For educational attainment, the observed site variation in impacts was so great that the probability it could have arisen by chance is virtually zero. However, for both employment and earnings, individual site impact estimates are less diverse, as is their precision (represented by a general lack of statistical significance on site impacts), so the probability that the observed variation could have arisen by chance is much higher: 72 percent for employment and 13 percent for earnings.

TABLE 6.3

SELECTED IMPACTS OF JOBSTART, BY SITE (NOT ADJUSTED FOR SITE DIFFERENCES IN SAMPLE CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT)

		Received GED or High School Diploma by End of Month 24			Ever Employed, Months 13-24			Total Earnings, Months 13-24		
Site	Sample Size	Experimentals	Controls	Difference	Experimentals	Controls	Difference	Experimentals	Controls	Difference
Concurrent										
Atlanta Job Corps	61	23.0%	8.6%	14.3	65.4%	76.5%	-11.0	$2,547.85	$4,853.46	-2,305.61*
CET/San Jose	152	34.5	27.4	7.1	90.0	86.3	3.7	7,827.81	7,319.39	508.42
Chicago Commons	74	5.6	18.7	-13.1	77.0	68.9	8.1	4,236.06	4,158.49	77.56
Connelley (Pittsburgh)	184	57.1	15.9	41.2***	68.5	58.5	10.0	1,637.28	2,093.65	-456.38
East LA Skills Center	100	2.1	8.0	-5.9	75.4	70.5	4.8	4,619.64	5,513.23	-893.59
EGOS (Denver)	183	22.2	14.8	7.4	63.8	65.1	-1.3	2,823.37	3,553.35	-729.99
Phoenix Job Corps	130	25.2	11.3	13.9*	71.4	81.2	-9.8	3,241.16	4,623.10	-1,381.95*
SER/Corpus Christi	236	53.2	15.1	38.1***	77.4	78.5	-1.2	3,684.59	3,575.35	109.23
Sequential/in-house										
El Centro (Dallas)	155	57.5	14.1	43.4***	77.5	65.2	12.3	3,926.87	3,363.75	563.12
LA Job Corps	218	19.8	14.8	5.1	64.3	62.2	2.1	3,960.76	4,418.62	-457.86
Sequential/brokered										
Allentown (Buffalo)	140	33.7	21.3	12.4	63.1	64.1	-0.9	2,843.72	2,370.42	473.31
BSA (NYC)	119	25.7	32.0	-6.3	67.1	74.8	-7.7	4,687.46	6,074.06	-1,386.61
CREC (Hartford)	87	21.8	12.4	9.4	69.2	64.0	5.2	4,333.44	4,627.33	-293.89
All sites	1,839	33.2	16.4	16.7***	72.0	69.5	2.5	3,894.08	4,098.81	-204.73

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for 19 kinds of difference in characteristics before random assignment; "all sites" outcomes are from a linear analysis of covariance procedure for the full sample controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE 6.4

SELECTED IMPACTS OF JOBSTART, BY SITE
(ADJUSTED FOR SITE DIFFERENCES IN SAMPLE CHARACTERISTICS
AT THE TIME OF RANDOM ASSIGNMENT)

Site or Statistic	Sample Size	Received GED or High School Diploma by End of Month 24 Experimentals	Controls	Impact	Ever Employed, Months 13-24 Experimentals	Controls	Impact	Total Earnings, Months 13-24 Experimentals	Controls	Impact
Concurrent										
Atlanta Job Corps	61	21.3%	10.0%	11.4	75.1%	78.5%	-3.4	$3,404.95	$5,323.75	-1,918.80*
CET/San Jose	152	33.6	25.9	7.7	81.4	74.8	6.6	6,946.06	6,206.79	739.27
Chicago Commons	74	6.0	15.9	-9.9	82.7	73.5	9.1	4,831.43	4,071.80	759.63
Connelley (Pittsburgh)	184	55.6	14.3	41.3***	74.3	63.2	11.1*	1,969.75	2,405.07	-435.33
East LA Skills Center	100	5.0	9.0	-4.0	70.4	67.8	2.6	4,509.77	5,203.43	-693.66
EGOS (Denver)	183	22.2	12.9	9.2	65.6	66.6	-1.0	2,998.48	3,581.51	-583.03
Phoenix Job Corps	130	26.0	11.1	14.9**	65.3	73.1	-7.8	3,005.02	3,575.39	-570.37
SER/Corpus Christi	236	55.4	15.9	39.6***	67.5	71.2	-3.8	2,581.26	2,643.74	-62.48
Sequential/in-house										
El Centro (Dallas)	155	57.9	17.1	40.8***	77.2	66.0	11.2	4,020.23	3,538.57	481.65
LA Job Corps	218	19.4	14.6	4.8	69.2	64.6	4.6	4,389.13	4,854.90	-465.77
Sequential/brokered										
Allentown (Buffalo)	140	30.3	20.8	9.5	72.4	73.9	-1.5	3,781.28	3,299.92	481.36
BSA (NYC)	119	29.5	29.0	0.5	72.1	72.8	-0.7	5,362.25	6,246.46	-884.21
CREC (Hartford)	87	22.7	15.3	7.4	73.8	69.3	4.5	5,024.72	5,261.38	-236.66
P-value of F-statistic				0.000***			0.724			0.767
All sites	1,839	33.2	16.4	16.7***	72.0	69.5	2.5	3,894.08	4,098.81	-204.73

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). "All sites" outcomes are from a linear analysis of covariance procedure for the full sample controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

234 235

Center (under both types of impact estimates) and at BSA in New York City (under the split-file approach). Sequential/brokered sites had the weakest educational attainment impacts, as one might expect from the generally higher *control group* receipt of a high school diploma or GED in these sites. This higher rate of control group receipt of a GED probably reflects the stronger interest in GED receipt among applicants in these sites, as discussed in an earlier section of this chapter, and the availability of alternative services in the two large cities of New York and Buffalo. El Centro in Dallas, SER/Corpus Christi, and Connelley in Pittsburgh's impacts were much larger than those for the other sites. There is a likely explanation for this that is related to program characteristics: These three sites placed great emphasis on GED attainment, in large part because of contractual provisions rewarding them for accomplishing this outcome.

None of the individual site impacts on second-year employment were statistically significant in the split-file table and only one was in Table 6.4, but there did appear to be variation within each site category (although it was not statistically significant). In both tables, each of the three categories showed both relatively positive impacts and one or more sites with negative or close to zero impacts. In both tables, the employment rate impacts were still negative in six of the sites, although in three of these sites, the difference between controls and experimentals was very small (less than 2 percentage points).

Impacts on second-year earnings showed more variation across sites than did the employment impacts; they also showed considerable variation within each of the three groups of sites. Because of the diverse earnings of youths within the individual sites and the small samples (owing to the fine level of disaggregation), only two of the 13 individual site impacts were statistically significant in Table 6.3 and only one was in Table 6.4. It is notable that there were positive and negative earnings impact estimates in each of the three groups of sites in both tables.

Earnings impacts in Tables 6.3 and 6.4 differ substantially for a few sites in the demonstration. The adjustments used in Table 6.4 make a larger difference for Chicago Commons than for all but one other site (the Phoenix Job Corps), complicating the comparison of impacts for Chicago Commons with other sites.[16] According to Table 6.3 (the split-file table), which does not equalize measured baseline characteristics across sites, the largest earnings impact estimate was $563.12 for El Centro in Dallas. Adjusting for site differences in sample characteristics, the El Centro earnings impact drops slightly to $481.65. Chicago Commons, in contrast, increases from a $77.56 impact estimate in Table 6.3 to $759.63, the largest earnings impact estimate in Table 6.4. This large change is attributable in part to the site's screening practices and training offerings, which led to unusual sample characteristics (especially a larger-than-average proportion of men).[17] Since Table 6.4 comes closer to making individual site impacts comparable, Chicago Commons should probably be viewed as producing positive, and relatively large, impacts.

It is also noteworthy that three of the four sites with positive estimates in Table 6.4 were among those described above as having implemented the components of JOBSTART most successfully. Chicago Commons and CET/San Jose had the largest point estimates for second-year earnings impacts in Table 6.4, but it is not apparent whether this was due to their relatively concentrated doses of JOBSTART, the strength of their job placement efforts, their integration of education and training, their longstanding reputations in their communities, or other factors unique to these sites, such as their close ties to employers. On a sixth important program feature, they differed: As noted above, CET/San Jose served applicants without screening, while Chicago Commons imposed strict entry requirements.[18]

The two other sites with positive earnings impacts in both tables (Allentown in Buffalo and El Centro in Dallas) operated different types of programs from CET/San Jose and Chicago

---

[16] The impacts of the Phoenix Job Corps become less negative with the shift from split-file to adjusted impacts, but neither this change nor the one for Chicago Commons affects the basic conclusions of the chapter. Another important site, CET/San Jose, had the second largest earnings impact in both tables, changing from $508.42 in Table 6.3 to $739.27 in Table 6.4.

[17] It also related to the relatively small sample at Chicago Commons (74 people) and to unusual site-specific coefficient estimates for the regression adjustments for experimental-control differences used in the split-file table. This change may also have been related to the small sample size, since in a small sample, outliers can have a strong influence on coefficient estimates.

[18] These two sites also differed somewhat in their experimental-control difference in service receipt, with CET/San Jose, at 52 percentage points, having a larger gap than Chicago, at 32 percentage points (see Table 6.2).

Commons.[19] Allentown had a heavy education focus (with only one-third of the sample getting any training), no integration of education and training, weak job placement efforts, and the longest average length of stay in the program (nearly four months above the average). It also had the smallest experimental-control service difference of any site. El Centro's sequential/in-house program rested somewhere in between these two extremes in both program emphasis and length of stay. Its service receipt difference may have played a crucial role in the positive impacts, being the largest of any site (72 percentage points).

Two sites assessed as implementing the JOBSTART program model well did not have positive earnings impacts in the second year of follow-up. Connelley in Pittsburgh did have strong educational attainment and employment rate impacts, but these had not yet translated into positive earnings impacts during the follow-up period. Connelley's long length of stay in the program (at 10 months, the second longest) may have been an important contributing factor. As to the second site, the Phoenix Job Corps, there is no clear reason for the lack of employment rate and earnings impacts.

III. Subgroup Impacts, by Site

This section continues the analysis by examining impacts on employment and earnings for key subgroups by site. At this fine level of disaggregation, the sample sizes in individual sites are so small as to make only the broadest conclusions possible. In view of the tentative nature of this analysis, tables are presented in Appendix G and the text presents the outlines of the findings. This analysis is based on site impacts adjusted for observed differences in pre-random assignment characteristics across the sites; that is, it uses the same approach as Table 6.4.

A. Second-Year Impacts for Men, by Site

The sample sizes for men at each site were usually less than half of the already small site sample sizes reported in Table 6.4. Since the full sample in each site yielded site differences in impacts that were not statistically significant, it is not surprising that most site-level impacts for men were also not statistically significant.

---

[19] A third site, SER/Corpus Christi, had a positive point estimate for the earnings impact according to the split-file method (Table 6.3) but not according to the adjusted method (Table 6.4) of calculating impacts.

With this caution in mind, the site story for men seems much the same as the full sample story. As in the full sample, employment and earnings impact differences across sites were not statistically significant, and not much should be made of them. However, the sites with substantial, positive point estimates for men were largely the same as the sites with positive estimates for the full sample. For example, CET/San Jose, Chicago Commons, and Allentown in Buffalo had point estimates of several hundred dollars for men, as was true of the full sample. However, there were two major exceptions to this pattern. First, El Centro in Dallas's impacts, which were positive for the full sample, were negative for men. Second, the largest estimate for impacts on earnings was for the 31 men at CREC in Hartford; the earnings impact in this site was driven by a 30 percentage point impact on employment.

In view of the fairly negative results for the full sample of men reported in Chapter 5, it is encouraging that there were sites with positive impacts for men on second-year earnings.

B. Second-Year Impacts for Women Living with Their Own Child(ren), by Site

Even more than for men or other women, site impacts for this group were based on very small samples in some sites, virtually assuring that site impacts and differences in impacts were not statistically significant; for example, there were only 10 mothers at CET/San Jose. Again bearing in mind that these impact estimates are shrouded in much uncertainty, the second-year labor market story for mothers seems a bit different from the stories for the full sample and for men.

The largest of the earnings impact estimates, for El Centro in Dallas mothers, apparently is what drove the full sample impact for El Centro. This impact was large enough to be statistically significant. The next largest earnings impact, at the East Los Angeles Skills Center, was driven by an employment rate impact of 49 percentage points. Unlike the earnings impact for men at the Los Angeles Job Corps, the earnings impact at that site for women living with their own child(ren) was positive. The earnings impact of more than $500 at CREC in Hartford for women living with their own children, combined with a large positive estimate for men, implies that the full sample impact for CREC must have been driven by a large negative estimate for other women. The positive but small earnings impacts among the 10 CET/San Jose mothers and the 63 Connelley in Pittsburgh mothers were driven by employment rate impacts of 50 percentage points and 22 percentage points, respectively. Earnings impacts in the other sites were negative, down to a statistically significant estimate of -$3,084 at BSA in New York City.

The findings for the three Job Corps sites for this subgroup hold special interest. Positive earnings impacts for women living with their own children, but not for other groups, at the Atlanta Job Corps and at the Los Angeles Job Corps contrast with earlier evaluation results for the residential Job Corps.[20] The residential Job Corps was found to be quite effective for men and for women without child care responsibilities, but not for mothers. These preliminary findings seem to suggest that a nonresidential Job Corps program may be more appropriate for mothers.

C. Second-Year Impacts for Other Women, by Site

The largest earnings point estimate was for the 14 "other women" (women who were not living with their own children) at Chicago Commons. Other positive impacts of more than $1,000 were achieved at the East Los Angeles Skills Center, the Los Angeles Job Corps, Connelley in Pittsburgh, and Allentown in Buffalo. El Centro in Dallas and CET/San Jose also had positive earnings impacts for these women. The other five sites had negative earnings impacts for these women during the second year. The biggest earnings loss for other women was at CREC in Hartford; thus, as noted above, it was these other women rather than mothers or men who were driving the negative overall earnings impact at CREC.

IV. Summary and Future Impacts To Watch

Despite strong policy interest in the influence of program structure, no simple story emerges. With respect to the full sample in each site, JOBSTART was sometimes effective and sometimes ineffective in yielding second-year labor market gains in brokered programs and in-house programs; it also showed varying success in both concurrent programs and sequential programs. This suggests that the JOBSTART program model can be implemented successfully in a variety of settings.

Adjusting for site differences in sample characteristics, Chicago Commons and CET/San Jose had the largest positive estimates for second-year earnings impacts. Each integrated education into the training sequence more than did other sites, but each also had several other features that distinguished it from most other sites, including strong job placement and relatively intense concentration of many instruction hours into a few months' length of stay. However, the two sites differed in the extent of initial screening of applicants, with CET/San Jose doing very little and Chicago Commons being among the sites with the most screening. Two other sites operating sequential/in-house and sequential/brokered programs (each with a much stronger education emphasis, and one providing very little training) also produced positive earnings impacts in the second year of follow-up.

---

[20] See Mallar et al., 1982.

The picture for subgroups is similarly mixed. Sites that had positive earnings impacts for the full sample generally had positive impacts for men and women considered separately. In contrast to negative findings from an earlier study of residential Job Corps mothers, JOBSTART mothers in two of three nonresidential Job Corps sites had earnings gains during the second year. These preliminary findings seem to suggest that a nonresidential Job Corps program may be more appropriate for mothers than a residential program.

Some JOBSTART sites (such as El Centro in Dallas) achieved both GED attainment gains and second-year earnings gains; others (the East Los Angeles Skills Center and at BSA in New York City) succeeded at neither. But the findings presented in this chapter provide evidence that sometimes there is a trade-off for program designers between emphasizing GED attainment and emphasizing accelerated occupational training for a specific job. In the two sites with the largest short-run adjusted earnings impacts – Chicago Commons and CET/San Jose – GED impacts were negative or fairly small. In the short run, emphasizing a specific skill may prove more effective in producing earnings impacts. However, the long-run picture could be quite different.

Education can create new opportunities, and these may arise over a long period of time. In comparison, training for a specific job opens a few doors quickly, but in the long run, because of economic change and uncertainty, they may not be the right doors. The oldest members of the JOBSTART sample were only 23 years old at the end of the follow-up available for this report; most sample members were considerably younger. Their transitions into full-time work and independent living arrangements will not be complete for several years. If the GED is important as a credential for better jobs or job training far into the future, labor market impacts at the two-year point may be quite misleading.

In the final JOBSTART report, using four years of follow-up, it will be especially important to look at impacts at the end of follow-up.

APPENDICES

APPENDIX A

DATA SOURCES FOR THE EVALUATION

Many data sources were used in this evaluation of the JOBSTART Demonstration. Baseline demographic data were collected at the time of random assignment. Management information system (MIS) data from the sites were used to measure participation hours. Twelve-month and 24-month follow-up surveys of applicants were conducted to measure impacts on experimentals (including those who did not participate) compared to controls; the impacts concerned amounts of education and training received, employment and earnings, and other outcomes. The 12-month survey also dealt with the experiences of participants in the JOBSTART program. Much qualitative information, including interviews with program staff as well as focus groups and in-depth interviews with participants, was used in conjunction with the quantitative information. Each data source is described below.

I. JOBSTART Enrollment Forms

The JOBSTART Enrollment Form, designed by MDRC and filled out by program staff at the time of random assignment, was the major source of information about the demographic and socioeconomic characteristics of sample members. It included data on age, sex, ethnicity, family composition, educational attainment, and time since dropping out of school, as well as basic information on welfare and employment histories. The enrollment form was completed for all but one sample member.[1]

II. JOBSTART Management Information System Forms

Sites used a number of MDRC-designed forms to report on the progress of participants in JOBSTART. The most important of these were:

---

[1] This sample member was excluded from the impact analysis, since all demographic variables from the enrollment form are missing. For many of the sample members, a few specific pieces of demographic information are missing. In the impact analysis, the predicted values based on similar sample members were substituted for these missing observations.

A. Monthly Participation Report

The Monthly Participation Report provided the number of hours that participants spent in basic education, occupational skills training, or other kinds of JOBSTART activities each month. It also provided information on the type of occupational skills training in which participants in training enrolled. Sites reported actual hours attended, not the number of hours scheduled.

Participation data used in this report were collected from August 1985 – the beginning of random assignment – through January 1989. The month of random assignment was included as a month of follow-up for participation, although the participant may have been randomly assigned late in the month. Those assigned in the last month of random assignment – November 1987 – had 15 months of follow-up participation data. The vast majority of the sample had at least 24 months of follow-up.

Collecting strictly comparable data across sites was not always possible, for two reasons: First, the services provided in each site varied; second, there was some inconsistency in the way sites reported hours for activities other than basic education or occupational training classes. For example, a number of sites supplemented education and/or training classes with formal classroom instruction in a variety of topics generally termed "life skills." Some sites reported these as education hours; others counted them as training hours. In order to have similar definitions of the basic components – education and training – MDRC modified the reported hours at sites, so that time spent in such activities as life skills classes was counted under "other activities."[2] The education hours reported by CET/San Jose also were adjusted to reflect only hours spent in the site's GED class.[3]

---

[2] The sites were El Centro in Dallas, the Los Angeles Job Corps, the Atlanta Job Corps, and Allentown in Buffalo. At El Centro, one-half of all education hours prior to December 1986 were spent in life skills. After 1986, one-fourth of the reported education hours were spent in life skills. The hours were counted as hours in "other activities" by MDRC. At the Los Angeles Job Corps, participants spent one-half of their reported education hours in activities such as art, gym, and "world of work" for the first three months after enrollment. MDRC moved one-half of the education hours to hours in "other activities" for those months. At the Atlanta Job Corps, 10 hours each week were spent in activities such as life skills, driver education, and health. MDRC moved 28.6 percent of the reported education hours to hours in "other activities." Allentown in Buffalo included such hours in its reported occupational training hours. MDRC moved all reported occupational training hours that did not have an associated type of training to hours in "other activities."

[3] CET/San Jose reported 30 percent of each participant's occupational training hours as education, which included time spent on training-related basic skills in occupational training courses as well as hours in the site's GED class. For consistency with other sites, the education and training hours at CET were

(continued...)

Other differences remained, however. A number of sites offered limited amounts of work experience as part of the JOBSTART program. Some sites reported these hours as training hours; others reported them under "other activities."[4] No adjustments were made in these hours. Finally, the Phoenix Job Corps did not report hours spent by participants in life skills or avocational activities, although the other two Job Corps sites did.

Appendix Table A.1 shows the common elements and variations in component activities across sites. In general, participation hours reported as being in the education component consisted of time spent in classes devoted to basic education or GED preparation; they did not include work on training-related basic skills done in occupational training courses. In all sites, participation hours that were counted in the training component included all activities offered in occupational training curricula, including units on training-related educational skills (such as Business English or Business Math) and employability development (instruction in work behaviors and job search). In the following sites, the hours counted as training also included time spent in work experience or on-the-job training: Connelley in Pittsburgh, El Centro in Dallas, the Phoenix Job Corps, and the Los Angeles Job Corps. Hours spent in "other activities" varied considerably across sites and included instruction in life skills, work experience, and orientation and avocational activities.

In order to assess the quality and completeness of the participation data, MDRC staff reviewed the teachers' class attendance records and other sources of data for a randomly selected sample of participants. For the most part, there was agreement between hours found in teachers' records and the Monthly Participation Reports. If more than 20 percent of the cases in a quality control sample had discrepancies greater than 10 percent between site-reported hours and hours obtained in the check, MDRC scheduled either a re-collection of the data or retraining of site staff, depending on the seriousness of the discrepancies.[5]

---

[3](...continued)
recalculated by MDRC, and only hours spent in the separate GED class were included as education hours in this report.

[4] At EGOS in Denver, hours spent by participants in "work study" were not reported.

[5] Because it was necessary to obtain records from a number of service providers, many of which did not maintain complete records for long periods, occupational training hours in brokered sites were the most difficult to confirm and probably have the greatest variation between actual and reported hours. The difficulty MDRC staff had in obtaining and verifying data from training providers reflects the difficulty sites had in monitoring hours for participants once they were no longer in the site. Problems were found even in the two sites with the best data from service providers: One site apparently over-reported hours, while one site apparently under-reported hours. Because the number of participants who entered training in
(continued...)

TABLE A.1

ACTIVITIES INCLUDED IN PARTICIPATION HOURS, BY SITE AND COMPONENT

Site	Education	Training	Other Activities
All sites	Classes in basic education or GED preparation	Classroom occupational skills training, including classes in training-related basic skills and employability development	Varies
Exceptions, by site			
Allentown (Buffalo)	---[a]		Life skills[a]
Atlanta Job Corps	---[a]		10-day orientation, work experience and on-the-job training, life skills and avocational activities[a]
BSA (NYC)	May include a few hours per week in computer-assisted life skills curriculum		Life skills
CET/San Jose	---[a]	---[a]	None
Chicago Commons			None
Connelley (Pittsburgh)		Work experience mentorships	None[b]
CREC (Hartford)	Includes some hours in employability development activities		Work experience internships
East LA Skills Center			None
EGOS (Denver)		---[c]	None
El Centro (Dallas)	---[a]	Work experience internships	Life skills[a]
LA Job Corps	---[a]	Work experience and on-the-job training	5-day orientation, life skills and avocational activities[a]
Phoenix Job Corps		Work experience and on-the-job training	8-day orientation[d]
SER/Corpus Christi			None

SOURCE: Adapted from Auspos et al., 1989.

NOTES: [a] Reported hours were adjusted by MDRC.

[b] Site did not report participation in a one-hour after-school component consisting of counseling and other support services in school year 1986-87.

[c] Site did not report participation hours in work-study positions.

[d] Site did not report participation hours in life skills and avocational activities.

B. Other Management Information System Data

As part of the monthly monitoring system, sites also reported on the end-of-month status of each participant, the participants who had been terminated and the reason for termination, and job placement and GED receipt among participants. The follow-up surveys proved to be a more complete source of data for employment and GED receipt, since they included activity by experimentals that might not have been reported to site operators as well as the experiences of the control group. Consequently, the surveys are the only source of these data used in this report.

III. Test of Adult Basic Education

The Test of Adult Basic Education (TABE), a modification of the California Achievement Test, was used to measure reading levels of experimentals. Prior research has shown the test to be a reliable and valid measure of reading ability. The test was used at two points in time: shortly after random assignment (as a baseline measure)[6] and after participants had spent some time in the program (usually after about 100 hours of education), as a measure of reading level gains.[7]

About 20 percent of the total experimental sample did not take a baseline TABE. The percentage tested varied by site from a high of 100 percent to a low of 42 percent. The Job Corps sites and CET/San Jose had the lowest percentage of experimentals with baseline TABEs.

---

[5] (...continued) sequential/brokered sites was small, the misreporting of training hours did not greatly affect the average hours of training presented in the report.

[6] In five sites, the TABE was also used as a test of reading-level eligibility and consequently was administered to controls as well as to experimentals. A number of other reading tests were administered in the other sites. Data from these sites were not included in the analysis because the data were not comparable across sites. Scores on the eligibility test were used as the baseline measure for experimentals in sites where the TABE was used.

[7] The actual number of hours of education between random assignment and the first follow-up test varied considerably because of differences in measuring hours of education and delays in administering the tests. Also, in the first few months of the demonstration, sites were asked to test every three months, which resulted in considerable variation in the number of hours after which participants were tested.

IV. Follow-Up Surveys

Eighty percent (1,839) of the 2,311 sample members randomly assigned to the experimental or control group between August 1985 and November 1987 were interviewed. (Table A.2 presents survey response rates by site.) These 1,839 youths constituted the sample for this report, and each of them provided follow-up information for 24 months after the date of his or her random assignment. Most responded to both the 12- and 24-month follow-up surveys (1,604 or 87 percent of responders), while the remainder responded to a special combination survey covering the entire 24 months, which was fielded for youths who did not respond to the 12-month survey but were located at 24 months. The surveys were conducted[8] either in person or, for the approximately one-fifth of the sample who had moved out of the area, by telephone, one and two years after random assignment. The interviews lasted about 45 minutes and provided information about the applicant's experience during the period of follow-up covered in that survey wave. Respondents were asked about their employment history, family status, welfare receipt, and receipt of education or training outside of JOBSTART. During the 12-month survey, experimentals who did not participate in JOBSTART were asked why; participants were asked what they like and disliked about the program and their reasons for leaving. (Appendix B discusses issues of sample bias and data quality for the survey.)

Sample members who could be located were generally willing to be interviewed. Some could not be located while others simply could not be contacted.

Some completed surveys lacked some information that was important in calculating impacts. Because the presence of missing data might have been correlated with an observed or unobserved prior attribute, dropping cases with missing data from the analysis might have biased the impact estimates or produced month-to-month inconsistencies. Imputing values is possible using a procedure that does not bias results. A separate regression was run for each variable with missing values, yielding predicted values for the missing data. These predicted values were used as estimates of the missing values. Continuous outcomes may contain outliers – extreme values that overly influence estimates. In the analysis, these were treated as missing, and the usual procedures for missing values were applied.

---

[8] MDRC contracted with Abt Associates, a Boston-based survey firm, to implement, manage, and monitor the survey. Completed surveys were data-entered and checked for completeness by Abt. Members of the Abt staff also assisted in the design of the survey instrument.

TABLE A.2

RESPONSE RATES FOR 24-MONTH SURVEY, BY SITE

Site	Sample Size	Responded	Did Not Respond
Concurrent			
Atlanta Job Corps	80	76.3%	23.8%
CET/San Jose	200	76.0	24.0
Chicago Commons	93	79.6	20.4
Connelley (Pittsburgh)	219	84.0	16.0
East LA Skills Center	126	79.4	20.6
EGOS (Denver)	237	77.2	22.8
Phoenix Job Corps	153	85.0	15.0
SER/Corpus Christi	300	78.7	21.3
Sequential/in-house			
El Centro (Dallas)	200	77.5	22.5
LA Job Corps	296	73.6	26.4
Sequential/brokered			
Allentown (Buffalo)	147	95.2	4.8
BSA (NYC)	151	78.8	21.2
CREC (Hartford)	109	79.8	20.2

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 2,311 sample members. Rows may not total 100.0 percent because of rounding.

V. Qualitative Data

Qualitative descriptions of the program and participants' experiences in it were obtained from a variety of sources and were used to complement the analysis of the quantitative data.

MDRC research staff visited sites and conducted structured interviews with program administrators, counselor/coordinators, and teaching staff to determine recruitment practices, the content of services in the education and training components, job placement and other activities, the range of support services and retention strategies, and staffing patterns and staff experience with JOBSTART. Staff also observed education and training classes in each site and visited some of the organizations that provided occupational training to JOBSTART participants in the sequential/brokered sites. Sites were typically visited by research staff once during the early phase of the demonstration and twice in the second year of program operations. This information was supplemented by ongoing reports on program operations and classroom observations provided by MDRC operations staff, who visited each site at regular intervals: once every month in year 1 and once every two months in year 2 of the operational period. (Interviews and observations concerning the education component were developed in conjunction with an education expert, who worked with MDRC as a consultant.)

Information about participants' reactions to JOBSTART was obtained from focus group discussions with 46 JOBSTART participants in four sites between May 1987 and February 1988. Female participants were interviewed at Connelley in Pittsburgh and at BSA in New York City; men were interviewed at El Centro in Dallas and at the Los Angeles Job Corps. Each session was attended by between 9 and 14 participants and lasted between two and two and a half hours. At Connelley and El Centro, the groups were made up of participants in attendance on the session day; at the Los Angeles Job Corps, staff selected students who were doing well in the program; the BSA group included both current participants in education and women who had already moved on to occupational skills training. Because they included many participants who stayed longer than the average and/or were doing well in the program, the groups were not representative of all JOBSTART participants. Nevertheless, used in conjunction with the survey responses, the focus group discussions provided valuable insights into participants' expectations about the program, what helped and hindered their participation, their opinions of the education and training components, and their recommendations for improving the program. MDRC hired consultants to develop the discussion topics, moderate the groups, and analyze the responses.

A series of in-depth interviews was conducted by another consultant with 15 JOBSTART participants in four other sites (CREC in Hartford, EGOS in Denver, Allentown in Buffalo, and the Atlanta Job Corps) between November 1986 and September 1987. These profiles provided additional, although impressionistic, information about the lives of some JOBSTART participants prior to and during the demonstration. The report also drew on the observations of JOBSTART staff and selected participants who attended a conference on Youth Employment Initiatives, sponsored by MDRC, in October 1987.[9]

---

[9] See Manpower Demonstration Research Corporation, 1988, for a summary of the conference discussions.

APPENDIX B

IMPACTS OF JOBSTART: METHODOLOGICAL ISSUES

As outlined in Chapter 2, several methodological issues had to be addressed to answer the key evaluation questions.

I. Selection Bias

Did random assignment succeed in creating a group of JOBSTART controls with the same pre-program characteristics as JOBSTART experimentals? If sample members become experimentals or controls completely at random, there are no systematic measured or unmeasured differences between the two groups before program treatment. Under those circumstances, average outcomes among controls measure what average outcomes would have been among experimentals had the treatment not been available to them, and the difference in average outcomes between experimentals and controls measures the program's effect. If there are systematic preexisting differences between experimentals and controls, then measured differences in post-treatment outcomes confound true program effects with biases due to the selection of more people from some groups to be experimentals and more people from other groups to be controls.

Table B.1 presents, one at a time, average characteristics for experimentals, controls, and both groups together. There were only slight differences between groups in a few individual characteristics, and no overall pattern of systematic differences between groups.

An alternative, more rigorous way to deal with this issue is to use linear regression analysis. To implement statistical tests for systematic experimental-control differences in those characteristics used in impact regressions, Table B.2 presents linear regression results measuring the extent of selection bias for the 2,311 members of the JOBSTART sample who filled out enrollment forms.[1] The first column of Table B.2 shows the same slight differences in individual characteristics and the same absence of systematic differences as did Table B.1. The final entry in the column, the p-value of the F-statistic, is very close to one, providing strong

---

[1] One sample member who did not complete an enrollment form was excluded from the impact analysis.

TABLE B.1

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY RESEARCH STATUS

Characteristic and Subgroups	Sample Size	Experimentals	Controls	Both Groups	p[a]
Gender					
Women	968	53.8%	51.3%	52.6%	0.284
Men	871	46.2	48.7	47.4	
Ethnicity					
White, non-Hispanic	155	7.6	9.3	8.4	0.340
Black, non-Hispanic	840	46.3	45.1	45.7	
Hispanic	783	43.3	41.8	42.6	
Other	61	2.8	3.8	3.3	
Ethnicity, by gender					
Women					
White, non-Hispanic	84	4.3	4.8	4.6	0.637
Black, non-Hispanic	445	25.2	23.1	24.2	
Hispanic	413	23.0	21.9	22.5	
Other	26	1.4	1.5	1.4	
Men					
White, non-Hispanic	71	3.3	4.5	3.9	
Black, non-Hispanic	395	21.1	21.9	21.5	
Hispanic	370	20.3	19.9	20.1	
Other	35	1.5	2.4	1.9	
Parental status					
Women living with own child(ren)					
No	484	27.5	25.1	26.3	0.311
Yes	484	26.3	26.3	26.3	
Men who have own child(ren)					
No	765	39.8	43.5	41.6	
Yes	106	6.3	5.2	5.8	
Employed within past year					
No	870	47.2	47.4	47.3	0.929
Yes	969	52.8	52.6	52.7	
Prior employment, by gender					
Women employed within past year					
No	547	29.7	29.8	29.7	0.538
Yes	421	24.1	21.6	22.9	
Men employed within past year					
No	323	17.5	17.6	17.6	
Yes	548	28.7	31.0	29.8	
Sample size	1,839	949	890		

(continued)

TABLE B.1 (continued)

Characteristic and Subgroups	Sample Size	Experimentals	Controls	Both Groups	p^a
Left school in grade 11 or 12					
No	1,078	57.7%	59.6%	58.6%	0.432
Yes	761	42.3	40.4	41.4	
Received occupational training within past year					
No	1,529	84.0	82.2	83.1	0.321
Yes	310	16.0	17.8	16.9	
Age					
16-19	1,359	74.1	73.7	73.9	0.857
20 or 21	480	25.9	26.3	26.1	
Marital status					
Ever married	174	9.3	9.7	9.5	0.775
Never married	1,665	90.7	90.3	90.5	
Living in own household or with boy/girlfriend					
No	1,500	82.4	80.7	81.6	0.340
Yes	339	17.6	19.3	18.4	
Own AFDC case or receiving General Assistance					
No	1,344	74.0	72.1	73.1	0.375
Yes	495	26.0	27.9	26.9	
Own AFDC case					
No	1,446	79.2	78.0	78.6	0.509
Yes	393	20.8	22.0	21.4	
Receiving Food Stamps					
No	1,143	62.0	62.4	62.2	0.860
Yes	696	38.0	37.6	37.8	
Arrested since age 16					
No	1,567	84.8	85.6	85.2	0.633
Yes	272	15.2	14.4	14.8	
Lived with both parents at age 14					
No	1,198	66.0	64.3	65.1	0.446
Yes	641	34.0	35.7	34.9	
Sample size	1,839	949	890		

(continued)

TABLE B.1 (continued)

Characteristic and Subgroups	Sample Size	Experimentals	Controls	Both Groups	p[a]
Site					
Concurrent					
Atlanta Job Corps	61	3.5%	3.1%	3.3%	1.000
CET/San Jose	152	7.9	8.7	8.3	
Chicago Commons	74	3.9	4.2	4.0	
Connelley (Pittsburgh)	184	9.9	10.1	10.0	
East LA Skills Center	100	5.4	5.5	5.4	
EGOS (Denver)	183	9.8	10.1	10.0	
Phoenix Job Corps	130	7.1	7.1	7.1	
SER/Corpus Christi	236	12.5	13.1	12.8	
Sequential/in-house					
El Centro (Dallas)	155	8.5	8.3	8.4	
LA Job Corps	218	12.1	11.6	11.9	
Sequential/brokered					
Allentown (Buffalo)	140	7.8	7.4	7.6	
BSA (NYC)	119	6.8	6.1	6.5	
CREC (Hartford)	87	4.7	4.7	4.7	
Sample size	1,839	949	890		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference in distributions of characteristics between groups: that is, p is the probability that observed proportions in each subgroup differ by research status only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE B.2

ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF ASSIGNMENT TO THE EXPERIMENTAL GROUP

Regressor or Statistic	Full Sample	24-Month Impact Sample	Combination-Survey Responders
Constant	0.503***	0.516***	0.464***
	(0.010)	(0.012)	(0.032)
Site			
Connelley (Pittsburgh)	-0.001	0.001	-0.077
	(0.054)	(0.060)	(0.175)
CET/San Jose	-0.004	-0.026	0.081
	(0.049)	(0.056)	(0.177)
SER/Corpus Christi	---	---	---
EGOS (Denver)	-0.000	0.002	0.083
	(0.045)	(0.052)	(0.176)
Chicago Commons	0.000	-0.003	-0.152
	(0.065)	(0.074)	(0.188)
El Centro (Dallas)	-0.003	0.026	0.258
	(0.051)	(0.058)	(0.215)
BSA (NYC)	0.002	0.054	0.207
	(0.054)	(0.062)	(0.152)
Allentown (Buffalo)	0.031	0.042	0.175
	(0.058)	(0.063)	(0.175)
CREC (Hartford)	0.003	0.012	-0.051
	(0.059)	(0.067)	(0.156)
Phoenix Job Corps	0.000	0.015	0.211
	(0.053)	(0.058)	(0.368)
East LA Skills Center	0.007	0.003	0.501**
	(0.055)	(0.062)	(0.205)
LA Job Corps	-0.006	0.024	0.192
	(0.047)	(0.054)	(0.134)
Atlanta Job Corps	-0.001	0.044	-0.176
	(0.069)	(0.079)	(0.290)
Male	-0.048*	-0.050	-0.042
	(0.027)	(0.031)	(0.091)

(continued)

TABLE B.2 (continued)

Regressor or Statistic	Full Sample	24-Month Impact Sample	Combination-Survey Responders
White, non-Hispanic	-0.023 (0.042)	-0.040 (0.048)	-0.352** (0.166)
Hispanic	0.018 (0.030)	0.028 (0.035)	0.072 (0.097)
Other ethnicity	-0.014 (0.068)	-0.073 (0.076)	-0.034 (0.162)
Age 20 or 21	0.010 (0.026)	-0.002 (0.029)	-0.063 (0.088)
No phone number on enrollment form	-0.063 (0.050)	-0.059 (0.057)	-0.265* (0.148)
Male parent	0.078 (0.047)	0.076 (0.054)	0.135 (0.134)
Female parent living with own child(ren)	0.008 (0.035)	-0.002 (0.039)	0.123 (0.113)
Limited English	-0.013 (0.057)	0.048 (0.066)	-0.076 (0.155)
Arrested since age 16	0.049 (0.037)	0.036 (0.042)	0.109 (0.102)
Convicted since age 16	-0.069 (0.054)	-0.023 (0.063)	-0.186 (0.181)
Own AFDC case	-0.012 (0.037)	-0.008 (0.041)	0.016 (0.122)
Receiving Food Stamps	-0.020 (0.030)	-0.007 (0.034)	-0.027 (0.090)
Never married	-0.010 (0.040)	0.019 (0.044)	0.090 (0.132)
Household AFDC case	0.069** (0.033)	0.070* (0.037)	0.010 (0.112)
Receiving Medicaid	-0.030 (0.031)	-0.027 (0.035)	0.043 (0.099)
Left school in grade 11 or 12	0.013 (0.023)	0.024 (0.025)	-0.016 (0.075)

(continued)

TABLE B.2 (continued)

Regressor or Statistic	Full Sample	24-Month Impact Sample	Combination-Survey Responders
Lived with both parents at age 14	-0.020 (0.023)	-0.012 (0.026)	-0.141* (0.082)
Employed within past year	-0.013 (0.023)	0.008 (0.025)	0.048 (0.074)
Number of observations	2,311	1,839	235
Number of experimentals	1,162	949	109
Number of controls	1,149	890	126
Degrees of freedom for error	2,279	1,807	203
Error mean square	0.251	0.252	0.245
R square	0.008	0.009	0.148
Mean of dependent variable	0.503	0.516	0.464
F-statistic	0.596	0.538	1.140
P-value of F-statistic	0.963	0.983	0.291

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: The dependent variable in each regression equation was unity for each experimental and zero for each control. Each characteristic on the right-hand side of each equation was measured as a deviation from its mean. The standard error of each coefficient estimate is enclosed in parentheses.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The p-value of the F-statistic is the probability of obtaining these coefficient estimates if the true chance of becoming an experimental did not vary with any characteristic. Thus, the closer the p-value is to unity, the more successful was random assignment in equating average characteristics of experimentals and controls.

evidence that there was no overall pattern of differences between experimentals and controls. It shows that random assignment created two groups without systematic overall differences in characteristics before enrollment. There were statistically significant differences in only two individual characteristics. For the full sample, experimentals were slightly less likely to be male and more likely to live in a household with someone else who received AFDC.

Among the 1,839 survey responders (see column two of Table B.2) and among the 235 combination survey responders (column three), the results of random assignment were similar. Although, judging from the statistically insignificant p-values for the survey responders, there were no systematic overall differences, the experimental survey responders were slightly more likely to live in a household with someone else who received AFDC. Combination survey responders were less likely to be white non-Hispanic, slightly less likely to have no phone number on their enrollment form, and slightly less likely to have lived with both parents at age 14. They were, however, more likely to have been at the East Los Angeles Skills Center.

The procedure used to calculate all the impacts presented in this report took these slight differences in characteristics into account, and estimated the impacts that would have occurred had these slight differences not existed.

II. Nonresponse Bias

Were those sample members for whom there are continuous data for 24 months representative of the full JOBSTART sample, including nonresponders? A high degree of mobility among disadvantaged young dropouts makes it difficult for survey interviewers to locate all of them a year or two after they have been enrolled into a research sample. As noted in Appendix A, 1,839 of the 2,311 full sample members furnished data covering 24 months, either at both the 12-month and 24-month junctures or at the 24-month juncture, for an overall response rate of 79.6 percent (81.7 percent for experimentals and 77.5 percent for controls).[2] See Table A.2 for site-specific information on response rates.

There were systematic differences in characteristics between those who responded to the surveys and those who did not respond. Table B.3 presents linear regression results measuring the extent to which average characteristics for the 1,839 survey responders differed from

---

[2] There are two types of nonresponse. Unit nonresponse is the failure to ascertain answers to any of the questionnaire items. Item nonresponse is the failure to obtain only some answers. All the response rates mentioned here are unit response rates.

TABLE B.3

ESTIMATED REGRESSION COEFFICIENTS FOR THE PROBABILITY OF UNIT SURVEY RESPONSE

	Sample and Dependent Variable	
Regressor or Statistic	24-Month Impact Sample Unit Survey Response 1,839/2,311	Combination Survey Unit Survey Response 235/1,839
Constant	0.796***	0.128***
	(0.008)	(0.008)
Experimental status	0.040**	-0.028*
	(0.017)	(0.015)
Site		
Connelley (Pittsburgh)	0.048	-0.023
	(0.043)	(0.039)
SER/Corpus Christi	---	---
CET/San Jose	-0.016	-0.060*
	(0.039)	(0.036)
EGOS (Denver)	-0.028	-0.056*
	(0.036)	(0.034)
Chicago Commons	0.011	0.039
	(0.051)	(0.048)
El Centro (Dallas)	-0.014	-0.084**
	(0.040)	(0.038)
BSA (NYC)	0.007	0.064
	(0.043)	(0.040)
Allentown (Buffalo)	0.168***	0.030
	(0.046)	(0.041)
CREC (Hartford)	0.017	0.065
	(0.047)	(0.044)
Phoenix Job Corps	0.061	-0.127***
	(0.042)	(0.038)
East LA Skills Center	0.025	-0.066*
	(0.044)	(0.040)
LA Job Corps	-0.061	0.129***
	(0.037)	(0.035)
Atlanta Job Corps	-0.034	-0.054
	(0.055)	(0.051)

(continued)

TABLE B.3 (continued)

	Sample and Dependent Variable	
Regressor or Statistic	24-Month Impact Sample Unit Survey Response 1,839/2,311	Combination Survey Unit Survey Response 235/1,839
Male	-0.027 (0.022)	0.035* (0.020)
White, non-Hispanic	-0.037 (0.034)	-0.018 (0.031)
Hispanic	-0.013 (0.024)	0.022 (0.023)
Other ethnicity	0.061 (0.054)	0.116** (0.049)
Age 20 or 21	0.001 (0.020)	-0.046** (0.019)
No phone number on enrollment form	-0.047 (0.040)	0.041 (0.037)
Male parent	-0.029 (0.038)	0.051 (0.035)
Female parent living with own child(ren)	0.032 (0.028)	0.022 (0.025)
Limited English	-0.038 (0.045)	0.013 (0.042)
Arrested since age 16	-0.002 (0.029)	0.072*** (0.027)
Convicted since age 16	-0.095** (0.043)	-0.071* (0.041)
Own AFDC case	-0.002 (0.029)	-0.016 (0.026)
Receiving Food Stamps	0.007 (0.024)	0.001 (0.022)
Never married	-0.008 (0.032)	0.002 (0.029)
Household AFDC case	-0.014 (0.026)	-0.005 (0.024)
Receiving Medicaid	-0.003 (0.025)	-0.002 (0.023)

(continued)

TABLE B.3 (continued)

	Sample and Dependent Variable	
Regressor or Statistic	24-Month Impact Sample Unit Survey Response 1,839/2,311	Combination Survey Unit Survey Response 235/1,839
Left school in grade 11 or 12	0.028	-0.035**
	(0.018)	(0.016)
Lived with both parents at age 14	0.050***	-0.007
	(0.019)	(0.017)
Employed within past year	0.011	-0.012
	(0.018)	(0.016)
Number of observations	2,311	1,839
Number of experimentals	1,162	949
Number of controls	1,149	890
Degrees of freedom for error	2,278	1,806
Error mean square	0.159	0.106
R square	0.034	0.070
Mean of dependent variable	0.796	0.128
F-statistic	2.482	4.214
P-value of F-statistic	0.000	0.000

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: The dependent variable in each regression equation was unity for survey response or combination-survey response and zero otherwise. Each characteristic on the right-hand side of each equation was measured as a deviation from its mean. The standard error of each coefficient estimate is enclosed in parentheses.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The p-value of the F-statistic in column one or column two is the probability of obtaining these coefficient estimates if the true chance of responding to the survey or to the combination survey did not vary with any characteristic. Thus, the closer the p-value is to zero, the more important are differences in characteristics between survey responders and nonresponders or between combination-survey responders and those who responded to only one of the two surveys.

average characteristics at random assignment for the 472 nonresponders. Since the final entry, the p-value of the F-statistic, is zero to three decimal places, there is strong evidence of systematic differences between responders and nonresponders. Responders were significantly less likely to have been convicted between the age of 16 and the time of random assignment, and significantly more likely to have lived with two parents at age 14. Also, better response was found at Allentown in Buffalo, even after taking differences in individual characteristics into account. In addition, responders were somewhat more likely to be experimentals than controls.

When nonresponse is randomly distributed among members of both treatment and control groups, it is troublesome only because it reduces the sample size and thus the statistical power to find impacts of a given size. Randomly distributed nonresponse does not alter the expected values of adjusted mean outcomes, and thus does not bias impacts. However, when nonresponse is greater among one research group (such as controls) or among members of either research group with certain characteristics (such as men), impacts may be biased slightly unless corrected for nonresponse. The most flexible correction for nonresponse is incorporation of an additional equation for survey response into a two-equation system with the impact equation. The success of attempts to implement such corrections is data-dependent, and the differential response rates found do not seem quite large enough to warrant such uncertain measures.

III. Impact of Participation Versus Impact of Assignment

Because the target population for the JOBSTART Demonstration consisted of young people who had histories of dropping out of education programs, it was difficult to get those selected for the program to attend and to retain attendees for substantial periods of time. However, everyone assigned to experimental status was included when calculating average impacts of JOBSTART. Therefore, impacts do not measure the impacts of participation in JOBSTART, but rather of assignment to the group eligible to receive JOBSTART services.[3]

---

[3] Some might suggest that nonparticipants be excluded from impact analyses. However, such exclusions would expose impacts to possible selection biases, undermining the control group's validity in measuring what would have happened without the program. When nonparticipants are excluded from the experimental group, average measured and unmeasured characteristics of experimentals may no longer be the same as average control group characteristics. See Cave, 1988.

Thus, impact estimates average net outcomes for all experimentals, including nonparticipants. Nonparticipation "waters down" the program effect the experiment seeks to detect. Fortunately, only 106 of the 949 experimentals in the impact sample never participated in the program. Such low nonparticipation may have been due in part to successful negotiation with sites to place the point of random assignment after initial assessment but immediately before program services started.

When substantial nonparticipation occurs during an experimental evaluation of a program, techniques are available for calculating impacts of participation as well as impacts of assignment. When the proportion of assignees to the program who are not counted as participants is an unbiased measure of the proportion of controls who would not have participated, when the program has no effect on nonparticipants, and when the sample is large enough, it is approximately valid to use the formula[4]

$$\text{Impact of participation} = \frac{\text{Impact of assignment}}{\text{Fraction participating}}.$$

Using this formula necessitates validating all of the assumptions underlying it, and thus makes impact analysis more complicated than a simple comparison of average outcomes for those assigned to the experimental group and those assigned to the control group. The assumption of zero effects on nonparticipants is troublesome, because the process of recruiting experimentals, screening them, and contacting them when they do not appear may alter their behavior. Thus, in this report, impacts of assignment were reported instead of impacts of participation.

As outlined above, impacts of assignment to JOBSTART were calculated by comparing average outcomes for all those assigned to the experimental group with average outcomes for all those assigned to the control group. In order to increase the statistical precision of the impact estimate, a variant of simple group averaging known as one-way linear analysis of covariance was used for the impact analysis in this report.[5] As shown for the full sample of 1,839 responders in Table B.4, in a multiple regression of outcome on covariates measured at

---

[4] See Cave, 1988; Auspos, Cave, and Long, 1988, Appendix E; Bloom, 1984; and Farkas et al., 1984, p. 85. If such an adjustment factor were appropriate here, its value would be approximately the reciprocal of the rate of participation in JOBSTART or 1 / (1 - 106/949) = 1.126.

[5] See Cave, 1987, and Ostle, 1975.

TABLE B.4

ESTIMATED REGRESSION COEFFICIENTS FOR SELECTED OUTCOMES

	Dependent Variable			
Regressor or Statistic	Ever Received Any Education or Training, Months 1-24 (%)	Received GED or High School Diploma by End of Month 24 (%)	Ever Employed, Months 13-24 (%)	Total Earnings, Months 13-24 ($)
Constant	44.236*** (1.286)	16.443*** (1.389)	69.523*** (1.420)	4,098.812*** (151.034)
Experimental status	48.418*** (1.794)	16.714*** (1.938)	2.473 (1.981)	-204.732 (210.715)
Site				
Allentown (Buffalo)	17.030*** (4.788)	-9.820* (5.171)	3.485 (5.286)	917.304 (562.332)
Atlanta Job Corps	0.973 (6.052)	-19.982*** (6.536)	6.922 (6.681)	1,659.399** (710.750)
BSA (NYC)	0.035 (4.752)	-7.046 (5.132)	2.701 (5.245)	3,142.873*** (558.026)
CET/San Jose	-20.218*** (4.251)	-5.824 (4.591)	8.737* (4.692)	3,948.982*** (499.199)
Chicago Commons	10.643* (5.608)	-24.383*** (6.056)	8.587 (6.190)	1,812.704*** (658.529)
Connelley (Pittsburgh)	4.810 (4.612)	-0.474 (4.981)	-0.795 (5.091)	-443.437 (541.634)
CREC (Hartford)	2.841 (5.138)	-16.617*** (5.549)	2.164 (5.671)	2,517.177*** (603.386)
East LA Skills Center	7.201 (4.740)	-28.807*** (5.119)	-0.369 (5.232)	2,238.462*** (556.584)
EGOS (Denver)	4.773 (3.967)	-18.154*** (4.284)	-3.365 (4.379)	670.499 (465.856)
El Centro (Dallas)	-4.543 (4.427)	2.323 (4.781)	2.198 (4.887)	1,177.852** (519.907)
LA Job Corps	-3.823 (4.123)	-19.036*** (4.453)	-2.649 (4.551)	1,988.714*** (484.163)
Phoenix Job Corps	-3.772 (4.422)	-17.826*** (4.776)	-0.550 (4.881)	674.547 (519.320)
SER/Corpus Christi	---	---	---	---
Age 20 or 21	-0.586 (2.188)	-1.782 (2.363)	-2.868 (2.415)	115.810 (256.968)

(continued)

TABLE B.4 (continued)

	Dependent Variable			
Regressor or Statistic	Ever Received Any Education or Training, Months 1-24 (%)	Received GED or High School Diploma by End of Month 24 (%)	Ever Employed, Months 13-24 (%)	Total Earnings, Months 13-24 ($)
Male	-8.165*** (2.341)	-0.803 (2.528)	17.626*** (2.584)	2,249.965*** (274.896)
Ethnicity				
White, non-Hispanic	-3.244 (3.633)	13.447*** (3.924)	14.017*** (4.010)	1,752.633*** (426.657)
Black, non-Hispanic	---	---	---	---
Hispanic	-1.887 (2.671)	-1.529 (2.885)	6.708** (2.948)	581.451* (313.667)
Other	4.504 (5.798)	-11.583* (6.262)	-1.077 (6.400)	964.634 (680.916)
Left school in grade 11 or 12	-1.804 (1.938)	6.357*** (2.093)	5.989*** (2.139)	819.836*** (227.585)
Limited English	-5.127 (5.000)	9.664* (5.400)	3.778 (5.520)	545.046 (587.227)
No phone number on enrollment form	-11.204*** (4.353)	-10.430** (4.701)	4.158 (4.805)	-290.917 (511.200)
Never married	-4.409 (3.381)	-0.809 (3.652)	-6.274* (3.732)	-982.086** (397.063)
Male parent	-3.261 (4.152)	-5.026 (4.484)	-3.077 (4.583)	629.133 (487.604)
Female parent living with own child(ren)	-7.048** (2.949)	-1.715 (3.185)	-9.240*** (3.255)	-320.469 (346.287)
Lived with both parents at age 14	0.768 (2.007)	0.732 (2.167)	4.965** (2.215)	680.035*** (235.652)
Own AFDC case	5.091* (3.096)	-0.790 (3.344)	-4.345 (3.418)	-244.139 (363.609)
Household AFDC case	4.717* (2.862)	-3.336 (3.091)	3.722 (3.159)	-108.485 (336.085)
Receiving Medicaid	-1.968 (2.697)	-0.236 (2.912)	-10.455*** (2.977)	-906.072*** (316.671)
Receiving Food Stamps	-1.989 (2.585)	5.231* (2.792)	-0.925 (2.853)	38.620 (303.572)

(continued)

TABLE B.4 (continued)

	Dependent Variable			
Regressor or Statistic	Ever Received Any Education or Training, Months 1-24 (%)	Received GED or High School Diploma by End of Month 24 (%)	Ever Employed, Months 13-24 (%)	Total Earnings, Months 13-24 ($)
Employed within past year	-2.521 (1.943)	0.081 (2.099)	5.370** (2.145)	1,001.186*** (228.190)
Arrested since age 16	-2.371 (3.187)	-2.763 (3.442)	-4.801 (3.518)	-384.891 (374.237)
Convicted since age 16	2.605 (4.835)	-0.232 (5.222)	-0.793 (5.337)	-377.090 (567.844)
Number of observations	1,839	1,839	1,839	1,839
Number of experimentals	949	949	949	949
Number of controls	890	890	890	890
Degrees of freedom for error	1,806	1,806	1,806	1,806
Error mean square	1,465.154	1,708.907	1,785.134	20,205,656.707
R square	0.325	0.107	0.152	0.203
Mean of dependent variable	69.222	25.068	70.799	3,993.162
F-statistic	27.13	6.73	10.12	14.36
P-value of F-statistic	0.000	0.000	0.000	0.000

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Ordinary least squares regression coefficients in this table correspond to impact estimates presented in Tables 4.1, 4.5, 5.1, and 5.4. A one-way linear analysis of covariance procedure was used to control for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; and Cave, 1987). The standard error of each coefficient estimate is enclosed in parentheses.

Each characteristic on the right-hand side of each equation was measured as a deviation from its mean.

A two-tailed t-test was applied to each coefficient estimate. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

the time of enrollment and on a dummy variable for research status, the coefficient of the dummy variable is the impact. This coefficient may be interpreted as the difference between the adjusted mean outcome for those assigned to the experimental group and the adjusted mean outcome for those assigned to the control group. Adjustment removes the effect of slight differences at the time of enrollment in characteristics related to the outcome, and yields a purer measure of the effect of research status alone.

Some of the subgroup results presented in the report were based on slightly more complex regression equations, which include terms for interactions between experimental status and subgroup characteristics. Such "two-way ANCOVA" impacts may differ to some extent from "split-file" impacts estimated by eliminating other subgroups from "one-way ANCOVA" analyses for Table B.4. However, calculating two-way ANCOVA impacts permits determining the statistical significance of impact differences, and is less burdensome computationally.

IV. The Internal Validity of Comparisons Among Subgroups and Types of Sites

Youths in the impact sample can be grouped based on their individual, pre-random assignment characteristics or on the characteristics of the sites at which they applied for JOBSTART. Because such subgroup comparisons are a central part of the analysis presented in this report, it is important to discuss briefly the complications in drawing conclusions from any observed differences. Crucial comparisons of this type are between men and women, and between types of sites. To summarize, impacts can be compared across subgroups defined by individual or site characteristics, but more caution is advised in interpreting such results than in interpreting the full sample or within-site impacts just described. This is especially true for comparisons of site types.

The basic reason is that, since sample members were not assigned randomly to these subgroups or types of sites, it may be impossible to isolate the difference in impact attributable to the single characteristic used to designate the groups. For example, using an example of a group "defined" by an individual characteristic, if women have bigger impacts than men, it may not be because they are women; the impact difference might really be because they had less prior work experience so controls were less likely to be working in the follow-up period. Further, using site or site groupings for a subgroup impact comparison is fundamentally different from using an individual characteristic such as gender. Many things about sites differ (such as labor market, participant characteristics and interests, and program characteristics), and there is a real danger that impact differences for site groupings may be misinterpreted as measuring the relative efficiency of a single feature of a site's program, such as its curricula, facilities, or program structure (that is, brokered versus in-house services or concurrent versus sequential education and training) rather than other factors that could be driving inter-site variation.

The internal validity of impact comparisons by individual characteristics is difficult to test. However, there is a simple test for internal validity of impact comparisons by program features. If groups of individuals randomly assigned at two locations really differed *only* in the features of the programs experimentals could attend, then the post-random assignment experience of controls at the two locations should be identical. This rarely happens; more typically, the experience of controls varies between sites, just as that of experimentals does.[6]

This problem can also affect impact comparisons for subgroups defined by characteristics of individuals as well as subgroup impacts by site or site grouping, although ordinarily the concerns about misinterpretation are less severe. For example, if virtually all the Hispanics recruited into a demonstration were concentrated in one or two sites, then "Hispanic" impacts really were impacts for "those sites." However, normally (and in the JOBSTART Demonstration, as discussed below) most measured characteristics of individuals are distributed fairly evenly across locations.[7] There will have been younger and older sample members, people reading at higher and lower levels, and parents and childless youths in samples recruited in all JOBSTART sites. Moreover, relevant unmeasured characteristics of individuals such as strength of motivation to attend a program and desire for a GED are likely to have been distributed fairly evenly among younger versus older sample members, those with higher versus lower reading levels, those who were parents versus those who were childless, and other subgroups defined by the observed characteristics of individuals.[8] If the impact on educational attainment, for example, was higher among low-reading-level than among high-reading-level sample members, it is reasonable to interpret this as evidence that the programs were more effective with the former rather than concluding that higher educational attainment occurred because the average sample member who had a low reading level was better motivated or wanted a GED more than did the average sample member who had a higher reading level.

---

[6] The usual situation is what is known in the evaluation literature as "ecological correlation bias."

[7] As discussed in the following section, several ethnic groups were highly concentrated in a few JOBSTART sites, however.

[8] To lessen this problem, impact estimates presented in this report always used site dummies as covariates when calculating impacts by individual characteristics; these dummies can correct for small differences between subgroups defined by individual characteristics in unmeasured characteristics associated with site.

In contrast, when a sample is split by site or site group, unmeasured characteristics will be distributed unevenly across groups. For example, sites that were known for providing education services and that offered a sequence of basic skills instruction followed by occupational training at another agency (sequential/brokered sites) may have been more likely to recruit clients motivated to get a GED than did concurrent sites with a reputation for training, whose typical client may have wanted to learn occupational skills. Thus, a finding that JOBSTART's impact on GED attainment was less at concurrent sites than at sequential/brokered sites does not necessarily mean that someone with average motivation and desire for a GED has a better chance of getting a GED in a sequential program. Such a finding could mean that those recruited in sequential locations had on average very different levels of desire for skills training relative to GED preparation from those recruited at concurrent locations. Even if the usual statistical adjustment methods are employed in calculating impacts, little can be done about this problem, since motivation to participate in particular components was not measured.[9]

---

[9] Subgroup impact equations for groupings by site type cannot use individual site dummies to correct for small unobserved differences in groups in the same way that equations for groups defined by individual characteristics can: Individual site dummies would be highly correlated with the site groupings. To measure which delivery system is better for those with average levels of motivation, desire for GEDs, and other unobserved characteristics, the best approach is to randomly assign people to each delivery system in each location after carrying out a common recruitment effort at that location. In that way, unmeasured characteristics would be the same for each delivery system, because each delivery system would be fairly represented in each location and in each recruitment effort. Other, nonexperimental approaches to this problem will be attempted in future stages of the JOBSTART research when longer follow-up is available.

APPENDIX C

SUPPLEMENTAL TABLES TO CHAPTER 2

TABLE C.1

CHARACTERISTICS IN THE YEAR BEFORE RANDOM ASSIGNMENT, BY EMPLOYMENT STATUS

Characteristic and Subgroups	Sample Size	Employed Within the Year Before Random Assignment		Both Groups	p^a
		No	Yes		
Gender					
Women	968	62.9%	43.4%	52.6%***	0.000
Men	871	37.1	56.6	47.4	
Ethnicity					
White, non-Hispanic	155	7.1	9.6	8.4**	0.015
Black, non-Hispanic	840	46.4	45.0	45.7	
Hispanic	783	42.0	43.1	42.6	
Other	61	4.5	2.3	3.3	
Ethnicity, by gender					
Women					
White, non-Hispanic	84	4.5	4.6	4.6***	0.000
Black, non-Hispanic	445	28.3	20.5	24.2	
Hispanic	413	28.3	17.2	22.5	
Other	26	1.8	1.0	1.4	
Men					
White, non-Hispanic	71	2.6	5.0	3.9	
Black, non-Hispanic	395	18.2	24.5	21.5	
Hispanic	370	13.7	25.9	20.1	
Other	35	2.6	1.2	1.9	
Parental status					
Women living with own child(ren)					
No	484	28.2	24.7	26.3***	0.000
Yes	484	34.7	18.8	26.3	
Men who have own child(ren)					
No	765	33.6	48.8	41.6	
Yes	106	3.6	7.7	5.8	
Prior employment, by gender					
Women employed within past year					
No	547	62.9	0.0	29.7***	0.000
Yes	421	0.0	43.4	22.9	
Men employed within past year					
No	323	37.1	0.0	17.6	
Yes	548	0.0	56.6	29.8	
Sample size	1,839	870	969		

(continued)

TABLE C.1 (continued)

Characteristic and Subgroups	Sample Size	Employed Within the Year Before Random Assignment: No	Employed Within the Year Before Random Assignment: Yes	Both Groups	p^a
Left school in grade 11 or 12					
No	1,078	59.1%	58.2%	58.6%	0.703
Yes	761	40.9	41.8	41.4	
Received occupational training within past year					
No	1,529	87.0	79.7	83.1***	0.000
Yes	310	13.0	20.3	16.9	
Age					
16-19	1,359	74.5	73.4	73.9	0.589
20 or 21	480	25.5	26.6	26.1	
Marital status					
Ever married	174	9.2	9.7	9.5	0.712
Never married	1,665	90.8	90.3	90.5	
Living in own household or with boy/girlfriend					
No	1,500	79.2	83.7	81.6**	0.013
Yes	339	20.8	16.3	18.4	
Own AFDC case or receiving General Assistance					
No	1,344	66.0	79.5	73.1***	0.000
Yes	495	34.0	20.5	26.9	
Own AFDC case					
No	1,446	70.6	85.9	78.6***	0.000
Yes	393	29.4	14.1	21.4	
Receiving Food Stamps					
No	1,143	58.0	65.8	62.2***	0.001
Yes	696	42.0	34.2	37.8	
Arrested since age 16					
No	1,567	88.5	82.2	85.2***	0.000
Yes	272	11.5	17.8	14.8	
Lived with both parents at age 14					
No	1,198	68.5	62.1	65.1***	0.004
Yes	641	31.5	37.9	34.9	
Sample size	1,839	870	969		

(continued)

TABLE C.1 (continued)

Characteristic and Subgroups	Sample Size	Employed Within the Year Before Random Assignment: No	Employed Within the Year Before Random Assignment: Yes	Both Groups	p[a]
Site					
Concurrent					
Atlanta Job Corps	61	2.6%	3.9%	3.3%***	0.000
CET/San Jose	152	6.4	9.9	8.3	
Chicago Commons	74	4.6	3.5	4.0	
Connelley (Pittsburgh)	184	6.1	13.5	10.0	
East LA Skills Center	100	6.3	4.6	5.4	
EGOS (Denver)	183	8.4	11.4	10.0	
Phoenix Job Corps	130	8.3	6.0	7.1	
SER/Corpus Christi	236	9.1	16.2	12.8	
Sequential/in-house					
El Centro (Dallas)	155	8.0	8.8	8.4	
LA Job Corps	218	18.9	5.6	11.9	
Sequential/brokered					
Allentown (Buffalo)	140	9.3	6.1	7.6	
BSA (NYC)	119	8.9	4.3	6.5	
CREC (Hartford)	87	3.1	6.2	4.7	
Sample size	1,839	870	969		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference in distributions of characteristics between groups: that is, p is the probability that observed proportions in each subgroup differ by employment status only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE C.2

CHARACTERISTICS IN THE YEAR BEFORE RANDOM ASSIGNMENT, BY GENDER AND EMPLOYMENT STATUS

Characteristic and Subgroups	Sample Size	Women Not Employed	Women Employed	Men Not Employed	Men Employed	All Categories	p^a
Ethnicity							
White, non-Hispanic	155	7.1%	10.7%	7.1%	8.8%	8.4%***	0.001
Black, non-Hispanic	840	45.0	47.3	48.9	43.2	45.7	
Hispanic	783	45.0	39.7	36.8	45.8	42.6	
Other	61	2.9	2.4	7.1	2.2	3.3	
Ethnicity, by gender							
Women							
White, non-Hispanic	84	7.1	10.7	0.0	0.0	4.6***	0.000
Black, non-Hispanic	445	45.0	47.3	0.0	0.0	24.2	
Hispanic	413	45.0	39.7	0.0	0.0	22.5	
Other	26	2.9	2.4	0.0	0.0	1.4	
Men							
White, non-Hispanic	71	0.0	0.0	7.1	8.8	3.9	
Black, non-Hispanic	395	0.0	0.0	48.9	43.2	21.5	
Hispanic	370	0.0	0.0	36.8	45.8	20.1	
Other	35	0.0	0.0	7.1	2.2	1.9	
Parental status							
Women living with own child(ren)							
No	484	44.8	56.8	0.0	0.0	26.3***	0.000
Yes	484	55.2	43.2	0.0	0.0	26.3	
Men who have own child(ren)							
No	765	0.0	0.0	90.4	86.3	41.6	
Yes	106	0.0	0.0	9.6	13.7	5.8	
Left school in grade 11 or 12							
No	1,078	58.9	61.0	59.4	56.0	58.6	0.449
Yes	761	41.1	39.0	40.6	44.0	41.4	
Sample size	1,839	547	421	323	548		

(continued)

TABLE C.2 (continued)

Characteristic and Subgroups	Sample Size	Women		Men		All Categories	p[a]
		Not Employed	Employed	Not Employed	Employed		
Received occupational training within past year							
No	1,529	89.4%	84.1%	83.0%	76.3%	83.1%***	0.000
Yes	310	10.6	15.9	17.0	23.7	16.9	
Age							
16-19	1,359	72.9	73.9	77.1	73.0	73.9	0.532
20 or 21	480	27.1	26.1	22.9	27.0	26.1	
Marital status							
Ever married	174	13.9	11.4	1.2	8.4	9.5***	0.000
Never married	1,665	86.1	88.6	98.8	91.6	90.5	
Living in own household or with boy/girlfriend							
No	1,500	68.9	74.3	96.6	90.9	81.6***	0.000
Yes	339	31.1	25.7	3.4	9.1	18.4	
Own AFDC case or receiving General Assistance							
No	1,344	55.0	68.2	84.5	88.1	73.1***	0.000
Yes	495	45.0	31.8	15.5	11.9	26.9	
Own AFDC case							
No	1,446	58.7	72.9	90.7	95.8	78.6***	0.000
Yes	393	41.3	27.1	9.3	4.2	21.4	
Receiving Food Stamps							
No	1,143	51.6	58.0	69.0	71.9	62.2***	0.000
Yes	696	48.4	42.0	31.0	28.1	37.8	
Arrested since age 16							
No	1,567	95.6	94.1	76.5	73.2	85.2***	0.000
Yes	272	4.4	5.9	23.5	26.8	14.8	
Sample size	1,839	547	421	323	548		

(continued)

TABLE C.2 (continued)

Characteristic and Subgroups	Sample Size	Women		Men		All Categories	p[a]
		Not Employed	Employed	Not Employed	Employed		
Lived with both parents at age 14							
No	1,198	70.7%	67.2%	64.7%	58.2%	65.1%***	0.000
Yes	641	29.3	32.8	35.3	41.8	34.9	
Site							
Concurrent							
Atlanta Job Corps	61	3.3	4.3	1.5	3.6	3.3***	0.000
CET/San Jose	152	6.2	9.7	6.8	10.0	8.3	
Chicago Commons	74	3.7	2.9	6.2	4.0	4.0	
Connelley (Pittsburgh)	184	6.2	15.2	5.9	12.2	10.0	
East LA Skills Center	100	5.1	3.8	8.4	5.3	5.4	
EGOS (Denver)	183	10.6	14.3	4.6	9.1	10.0	
Phoenix Job Corps	130	8.6	4.8	7.7	6.9	7.1	
SER/Corpus Christi	236	9.3	10.0	8.7	21.0	12.8	
Sequential/in-house							
El Centro (Dallas)	155	7.9	9.7	8.4	8.0	8.4	
LA Job Corps	218	18.8	6.4	18.9	4.9	11.9	
Sequential/brokered							
Allentown (Buffalo)	140	10.1	6.4	8.0	5.8	7.6	
BSA (NYC)	119	6.4	4.3	13.0	4.4	6.5	
CREC (Hartford)	87	3.8	8.3	1.9	4.6	4.7	
Sample size	1,839	547	421	323	548		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a]The column labeled "p" is the statistical significance level of differences among groups in distributions of characteristics: that is, p is the probability that observed proportions in each subgroup differ from one column to another only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE C.3

CHARACTERISTICS AT THE TIME OF RANDOM ASSIGNMENT, BY AGE

Characteristic and Subgroups	Sample Size	Age 16-19	Age 20 or 21	Both Groups	p^a
Gender					
Women	968	52.2%	53.7%	52.6%	0.570
Men	871	47.8	46.2	47.4	
Ethnicity					
White, non-Hispanic	155	9.8	4.6	8.4***	0.000
Black, non-Hispanic	840	42.6	54.4	45.7	
Hispanic	783	44.3	37.7	42.6	
Other	61	3.3	3.3	3.3	
Ethnicity, by gender					
Women					
White, non-Hispanic	84	5.1	3.1	4.6***	0.000
Black, non-Hispanic	445	21.7	31.2	24.2	
Hispanic	413	24.0	18.1	22.5	
Other	26	1.5	1.2	1.4	
Men					
White, non-Hispanic	71	4.7	1.5	3.9	
Black, non-Hispanic	395	20.9	23.1	21.5	
Hispanic	370	20.3	19.6	20.1	
Other	35	1.8	2.1	1.9	
Parental status					
Women living with own child(ren)					
No	484	30.3	15.0	26.3***	0.000
Yes	484	21.9	38.7	26.3	
Men who have own child(ren)					
No	765	43.9	35.2	41.6	
Yes	106	3.9	11.0	5.8	
Employed within past year					
No	870	47.7	46.2	47.3	0.589
Yes	969	52.3	53.7	52.7	
Prior employment, by gender					
Women employed within past year					
No	547	29.4	30.8	29.7	0.532
Yes	421	22.9	22.9	22.9	
Men employed within past year					
No	323	18.3	15.4	17.6	
Yes	548	29.4	30.8	29.8	
Sample size	1,839	1,359	480		

(continued)

TABLE C.3 (continued)

Characteristic and Subgroups	Sample Size	Age 16-19	Age 20 or 21	Both Groups	p^a
Left school in grade 11 or 12					
No	1,078	61.4%	50.6%	58.6%***	0.000
Yes	761	38.6	49.4	41.4	
Received occupational training within past year					
No	1,529	83.4	82.5	83.1	0.662
Yes	310	16.6	17.5	16.9	
Marital status					
Ever married	174	7.4	15.2	9.5***	0.000
Never married	1,665	92.6	84.8	90.5	
Living in own household or with boy/girlfriend					
No	1,500	86.3	68.1	81.6***	0.000
Yes	339	13.7	31.9	18.4	
Own AFDC case or receiving General Assistance					
No	1,344	78.7	57.3	73.1***	0.000
Yes	495	21.3	42.7	26.9	
Own AFDC case					
No	1,446	83.0	66.2	78.6***	0.000
Yes	393	17.0	33.7	21.4	
Receiving Food Stamps					
No	1,143	65.0	54.0	62.2***	0.000
Yes	696	35.0	46.0	37.8	
Arrested since age 16					
No	1,567	85.2	85.2	85.2	0.999
Yes	272	14.8	14.8	14.8	
Lived with both parents at age 14					
No	1,198	65.7	63.5	65.1	0.392
Yes	641	34.3	36.5	34.9	
Sample size	1,839	1,359	480		

(continued)

TABLE C.3 (continued)

Characteristic and Subgroups	Sample Size	Age 16-19	Age 20 or 21	Both Groups	p[a]
Site					
Concurrent					
Atlanta Job Corps	61	3.5%	2.9%	3.3%***	0.000
CET/San Jose	152	8.8	6.7	8.3	
Chicago Commons	74	2.6	8.1	4.0	
Connelley (Pittsburgh)	184	7.4	17.3	10.0	
East LA Skills Center	100	5.7	4.6	5.4	
EGOS (Denver)	183	10.3	9.0	10.0	
Phoenix Job Corps	130	8.2	3.7	7.1	
SER/Corpus Christi	236	12.2	14.6	12.8	
Sequential/in-house					
El Centro (Dallas)	155	9.6	5.2	8.4	
LA Job Corps	218	12.4	10.2	11.9	
Sequential/brokered					
Allentown (Buffalo)	140	7.6	7.7	7.6	
BSA (NYC)	119	6.5	6.5	6.5	
CREC (Hartford)	87	5.2	3.5	4.7	
Sample size	1,839	1,359	480		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all 1,839 sample members for whom there were 24 months of follow-up survey data. Sample sizes reported may fall short of this number because of items missing from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference in distributions of characteristics between groups: that is, p is the probability that observed proportions in each subgroup are different only because of random error. A Pearson chi-square statistic was used to test the hypothesis of equal distributions. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

APPENDIX D

SUPPLEMENTAL TABLE TO CHAPTER 3

TABLE D.1

RATINGS AND SPECIAL FEATURES OF THE IMPLEMENTATION OF JOBSTART COMPONENTS, BY SITE

	Ratings and Special Features of Components			
Site	Education	Training	Support Services	Job Placement
Concurrent				
Atlanta Job Corps	Rating: *Medium* ·self-paced, individualized instruction supplemented by group instruction ·some computer-assisted instruction ·unclear objectives ·frequent staff turnover	Rating: *Medium* ·good mix of on- and off-site training ·lacked state-of-the-art equipment ·some employer and union presence	Rating: *High* ·incentives for participation ·on-site child care ·strong counseling ·health services	Rating: *Low* ·few linkages to employers ·little special attention devoted to JOBSTART youths
CET/San Jose	Rating: *None* ·a rating of this component is inappropriate because education and training were more integrated than in other sites, although there was separate GED preparation	Rating: *High* ·open entry and exit ·self-paced, individualized instruction ·on-site training ·concurrent education and training ·instructors hired from industries about which they teach ·participants had opportunity to observe different training classes before making a selection	Rating: *Medium* ·on-site child care; not subsidized, but sliding scale ·little individualized counseling; most provided by instructors ·no incentive payments	Rating: *High* ·good relations with employers ·job placement a high priority

(continued)

TABLE D.1 (continued)

Site	Ratings and Special Features of Components Education	Training	Support Services	Job Placement
Chicago Commons	Rating: *Medium* ·rigorous curricula coordinated with vocational requirements, allowing for immediate application and reinforcement of learning ·special tutorial workshops for JOBSTART participants ·no computer-assisted instruction ·because of vocational focus, GED preparation almost precluded	Rating: *High* ·training provided in word processing and in 4 semiskilled industrial trades ·rigorous curricula that incorporated input from employers ·hands-on instruction almost daily ·certified instructors recruited from work settings students were preparing to enter ·state-of-the-art equipment ·instructors adult-oriented, not always sensitive to youths' developmental needs	Rating: *Medium* ·relatively small caseloads, but counselors carried full responsibility for recruitment during periods ·referrals for child care ·JTPA needs-based payments for transportation and lunch, but checks were frequently late ·adult-oriented environment	Rating: *High* ·staffperson fully dedicated to job development ·extensive ties to employer community ·all placements training-related ·given demanding nature of potential placements, youths needed more exposure to employment environment prior to job interviews (began visits to workplaces during later months of the demonstration's operating period)
Connelley (Pittsburgh)	Rating: *High* ·clear objectives ·excellent infrastructure ·computer-assisted instruction ·combined occupational training with basic education and GED preparation in some cases ·tutorial assistance ·multimedia approach	Rating: *High* ·multiple training areas ·combined basic skills and vocational training ·interactive learning ·strong employer presence	Rating: *High* ·referrals to other agencies ·on-site child care ·needs-based payments and incentives ·special JOBSTART counselors ·some clothing assistance ·special retention activities	Rating: *Medium* ·informal, individualized job search ·too few job developers to provide focused assistance to JOBSTART youths ·poor attention to world of work/job-readiness

(continued)

TABLE D.1 (continued)

	Ratings and Special Features of Components			
Site	Education	Training	Support Services	Job Placement
East LA Skills Center	Rating: *Medium* ·clear objectives ·licensed instructors ·individualized instruction supplemented by group instruction ·some effort to combine occupational training and GED preparation ·some use of the computer lab	Rating: *Medium* ·good mix of training areas on-site ·some staff turnover and some uninspired instructors ·combined occupational training with basic education and GED preparation ·good relations with employers ·crowded classrooms made it difficult for some youths to get the attention of instructors	Rating: *Medium* ·counseling provided ·no incentives for participation ·limited transportation, meal, or financial assistance	Rating: *Low* ·some supervised, individualized job search ·one placement staff-person ·little attention to JOBSTART youths
EGOS (Denver)	Rating: *Medium* ·open entry and exit ·self-paced, individualized instruction ·JOBSTART-only classes ·on-site classes ·on-site GED testing with almost immediate reporting of results ·computer-assisted instruction available, but not widely used	Rating: *Medium* ·not always open entry and exit ·several training programs required more time than participants had available ·training staff used to teaching adults, not always pleased to have JOBSTART participants in class	Rating: *Medium* ·child care payments ·free bus passes ·trained, caseload-carrying counselors for participants ·referrals for other services ·good attendance enforcement ·no incentives to participate except occasional parties	Rating: *Low* ·no specific job development staff ·few successful completions of training and transitions to employment

(continued)

TABLE D.1 (continued)

	Ratings and Special Features of Components			
Site	Education	Training	Support Services	Job Placement
Phoenix Job Corps	Rating: *Medium* ·individualized, competency-based instruction ·computer-assisted instruction ·open entry and exit ·little opportunity for group instruction/ interaction	Rating: *High* ·individualized, competency-based instruction ·open entry and exit ·wide range of courses available ·certified instructors ·little opportunity for group instruction/ interaction	Rating: *High* ·financial incentives for attendance and performance ·structured procedures for reviewing participants' performance ·youth-oriented environment ·relatively large caseloads ·referrals for child care, but limited community resources	Rating: *Low* ·several staffpersons fully dedicated to job development ·job development services offered to non-completers in reasonably good standing at program departure ·many placements not training-related
SER/Corpus Christi	Rating: *High* ·well-articulated curriculum and weekly plans ·used computer-assisted instruction to supplement GED preparation ·well-articulated attendance and punctuality standards ·some effort to integrate vocational instruction with GED preparation ·clear performance outcomes were attained	Rating: *Low* ·on-site training, but limited to clerical and automotive skills ·lacked state-of-the-art equipment ·difficulty attracting and retaining qualified staff ·limited linkages with the private sector ·maintained fairly high student morale and attendance despite shortcomings	Rating: *High* ·needs-based payments for transportation and meals ·referrals for child care to other SER locations or other providers ·incentive payments ·strong counseling	Rating: *Low* ·responsibility for placement rested with Texas Employment Commission, which did not consider placement of JOBSTART youths a priority

(continued)

TABLE D.1 (continued)

	Ratings and Special Features of Components			
Site	Education	Training	Support Services	Job Placement
Sequential/in-house				
El Centro (Dallas)	Rating: *High* ·clear objectives ·well-designed curriculum ·good mix of individualized and group instruction ·efforts to integrate vocational curriculum ·tutorial assistance ·reading and learning lab for those needing extra help ·flexible program ·integration of field trips and current events with GED preparation ·multimedia approach ·licensed instructors ·community college infrastructure	Rating: *Medium* ·clear objectives ·7 on-site training areas ·certified instructors with employer contacts ·state-of-the-art equipment in cable TV and clerical training; fairly current equipment in other areas ·interactive learning ·good curriculum	Rating: *Medium* ·needs-based payments ·bus passes ·incentive payments ·some emergency rent assistance ·good counseling	Rating: *Medium* ·job placement staff responsible for all El Centro students ·limited attention to JOBSTART youths
LA Job Corps	Rating: *Medium* ·clear objectives ·stable staff ·multimedia approach ·interactive learning	Rating: *Medium* ·good mix of training areas ·stable staff ·good employer and union presence ·state-of-the-art equipment in most areas ·on- and off-site linkages	Rating: *High* ·incentives ·strong counseling ·health and other services	Rating: *Medium* ·good relations with employers ·some supervised job search ·limited attention to JOBSTART youths

(continued)

TABLE D.1 (continued)

Site	Education	Training	Support Services	Job Placement
	Ratings and Special Features of Components			
Sequential/brokered				
Allentown (Buffalo)	Rating: *High* ·computer-assisted instruction ·individualized instruction supplemented by group instruction ·interactive learning ·GED-preparation focus	Rating: *Low* ·difficulty in transition of youths to JTPA and proprietary schools ·no on-site training	Rating: *High* ·counseling ·needs-based payments ·meals and transportation ·referrals for medical, dental, and other services ·no incentive payments for participation	Rating: *Low* ·limited direct placement assistance provided to JOBSTART youths ·reliance on placement staff at training agencies
BSA (NYC)	Rating: *Medium* ·good mix of academic, job-readiness, and life skills activities ·computer-assisted instruction ·fairly limited GED preparation	Rating: *Low* ·limited referrals ·lack of success linking up with JTPA	Rating: *Medium* ·strong counseling ·strong relationship between staff and participants ·some needs-based payments for a portion of the participants ·referrals for other services ·incentive payments for part of demonstration	Rating: *Low* ·few successful completions of training and transitions to employment ·limited involvement with employers

(continued)

TABLE D.1 (continued)

	Ratings and Special Features of Components			
Site	Education	Training	Support Services	Job Placement
CREC (Hartford)	Rating: *Low* ·computer-assisted instruction ·limited GED preparation ·underutilized computer learning center ·limited use of group instruction ·attrition and low attendance	Rating: *Low* ·limited access to training ·poor linkages to employers ·few transitions to training	Rating: *Medium* ·counseling ·bus passes ·referrals for other services ·no needs-based payments ·no incentive payments for participation	Rating: *Low* ·limited involvement with training or employer community ·limited attention to JOBSTART youths ·informal job search assistance

SOURCE: Observations of MDRC operations and research staff during the period of program operation in each site.

APPENDIX E

COST OF THE JOBSTART PROGRAM

I. General Approach

This appendix describes the data sources and methodology used to estimate the cost of the JOBSTART program in each of the 13 demonstration sites. It also discusses the factors contributing to the wide variation in costs across sites and examines the relative influence of different JOBSTART components on overall program costs.

The central objective of the analysis was to identify the market value of *all* resources used in providing JOBSTART services. It therefore counted as program costs not only the expenditures made by the agencies sponsoring the program, but also those made by outside organizations responsible for providing certain components (such as occupational skills training in the three sequential/brokered sites). Furthermore, in sites where goods and services that affected the nature of the program treatment were donated to the sponsoring agency, the analysis estimated the market value of those contributions and counted it, too, as a program cost.[1] For these reasons, the costs presented here may differ from those reflected in a sponsoring agency's own fiscal records.

This appendix does not present estimates of the cost of education and training services received by members of the control group. Thus, it provides no insights into the *incremental* investment that the JOBSTART sites made for the experimental group. An estimate of these incremental or "net" costs would be an important part of a full benefit-cost analysis, where the value of experimental-control differences in earnings and other outcomes (the "benefits") is compared with the experimental-control differences in the cost of services producing those benefits. As Chapter 4 showed, a substantial proportion of the control group did receive education and training services during the research follow-up period, which means that the net cost of JOBSTART in some sites may be considerably smaller than the gross cost reported here.

---

[1] For example, at Chicago Commons, many of the basic supplies essential to operating some of the training courses were donated to the program. The estimated value of these supplies, as reported in the agency's annual audit report, was thus counted as a program cost. As another illustration, the life skills workshops at both Chicago Commons and Connelley in Pittsburgh were conducted free of charge at the program site by outside organizations. These donated services were thus valued and included in the total cost. Their estimated value was based on the number of sessions conducted and by a proxy value of the average cost per session to the agency providing the service.

A. Data Sources and Accounting Periods

Data for the cost analysis were gathered from a variety of sources. These include:

- individual staff salary information;
- site expenditure reports, which showed overall expenditures on salaries and fringe benefits, rent, utilities, supplies, equipment, administration, and so on;
- program enrollment and participation data covering JOBSTART and non-JOBSTART participants, both for the program as a whole and for individual components (such as education classes and training classes);
- JTPA expenditure data in sites where JTPA funds were used to provide program services;
- agency data on support service expenditures covering needs-based payments, transportation, food, child care, and other participant payments;
- interviews with program staff concerning the allocation of staff time across program components and between JOBSTART and non-JOBSTART functions, and other aspects of site operations that affected the use of resources; and
- MDRC's MIS data on the experimental group's degree of participation in JOBSTART activities.

In most cases, data from these sources covered a one-year "steady-state" period sometime between 1985 and 1988 (depending on the site), the years during which JOBSTART was funded.[2] However, the actual calendar months of this accounting period varied according to each site's date of entry into the demonstration and the particular months covered by its annual fiscal reporting period.[3]

Ideally a steady-state period should reflect a time during which program operations are relatively stable. Although it was difficult to define such a period for JOBSTART because of the demonstration's relatively short duration, the period selected in most sites began at least several months after the initiation of the project (in order to avoid the start-up costs associated with beginning a new program), and ended at least several months prior to the termination of the demonstration (in order to exclude the phasedown period).[4] To remove the influence of inflation resulting from the use of costs from different calendar periods in different sites, all estimates were inflated or deflated to 1986 dollars.

---

[2] The JOBSTART program at Connelley in Pittsburgh and SER/Corpus Christi changed substantially from the first year of operations to the next. Consequently, cost and participation data for both years were used.

[3] In some sites, participation data and expenditure reports did not cover exactly the same time period, so a number of additional adjustments had to be made in estimating average steady-state expenditures.

B. Excluded Costs

In estimating the average cost per JOBSTART experimental, adjustments were made to exclude two categories of expenditures embedded in the sites' fiscal data: (1) research-related costs, and (2) the costs of services or activities that were offered to or used by non-JOBSTART participants. A fraction of program expenditures during the steady-state period resulted exclusively from research requirements. These included the extra costs involved in recruiting and processing individuals who became part of the control group, as well as the costs of staff time spent on conducting random assignment, completing the research enrollment forms, and participating in research-related interviews with MDRC personnel. These activities are not part of the normal effort of operating a JOBSTART program. Thus, the resources spent on them are not counted in the average cost estimates reported here.

Several of the sites also offered a number of services that were not a part of the JOBSTART program but were nonetheless captured in the agencies' aggregate expenditure reports. These, too, had to be excluded from the estimates of JOBSTART costs. This issue was most significant in the three Job Corps sites, where some Corpsmembers lived in dormitories at the centers, while others lived at home while attending Job Corps activities. All of the JOBSTART experimentals were nonresidents in these sites, and were thus unaffected by the services intended exclusively for the residents. These residential-only services included: dormitory provisions, most night-time and weekend recreational activities,[5] and supervision by residential advisors and dormitory attendants. The share of total Job Corps costs associated with exclusively residential aspects of the program was thus estimated and eliminated from the resources counted in determining the cost of JOBSTART.[6]

---

[4] Because the core education and training services in most sites were already in place prior to JOBSTART and continued after the demonstration, start-up and phasedown costs for the JOBSTART Demonstration were not an issue for these components.

[5] Although nonresidents were invited to participate in all recreation activities, they did so much less frequently than residential Corpsmembers.

C. Calculating the Average Cost per Experimental

In each site, the total average cost of JOBSTART per experimental was determined by summing the average cost of several relatively distinct program components and services. Determining these component costs involved several steps. First, an average unit cost during the steady-state period – that is, the cost of serving one person in the component for a specified unit of time – was calculated. The unit of measure varied for some components, mostly depending on whether the activity operated on an open-entry/open-exit or a fixed-cycle basis.[7] Thus, for open-entry components, the average cost of serving one person for one month in the activity was estimated; for fixed-cycle activities, the average cost per person who ever entered a given cycle of the activity was estimated.

The numerator in these unit costs incorporated total expenditures for personnel and overhead functions, including expenses incurred for non-JOBSTART participants in sites where the experimentals were enrolled along with other persons in regular agency activities.[8] The value of donated goods and services was also counted as a program expenditure. The denominator includes all participants (both JOBSTART and non-JOBSTART) in the component. Thus, for example, the unit cost of basic education at a site is the full cost of classes in which JOBSTART participants were enrolled, divided by the total number of students in the classes.

The average unit cost was then multiplied by a corresponding participation measure.[9] For open-entry components, the unit cost was multiplied by the average number of months in which experimentals spent any hours in the activity (including zero months for experimentals with no hours in the activity).[10] For components that operated on a fixed-cycle basis, the unit cost (the cost per person who entered the activity) was multiplied by the proportion of JOBSTART experimentals who ever entered the activity.[11] The values for both types of participation measures were based on the experiences of the experimentals in the "impact sample" (the sample of survey responders used in this report), not of all experimentals who were randomly assigned.[12]

---

[6] A technical assistance project at BSA in New York City is another example of separate activities whose costs had to be excluded in estimating JOBSTART costs.

[7] When participants leave open-entry activities, they are typically replaced by other individuals. Thus, the average value of resources expended per person for these components varies with the length of time an average participant receives that service. However, if entry into a training class is based on a fixed cycle and a student who drops out is not replaced by another student, the costs for that student's "slot" are still incurred by the agency on that student's behalf, regardless of his or her length of stay.

[8] For some activities, sites mainstreamed JOBSTART participants with non-JOBSTART participants. Other activities (for example, counseling, life skills instruction, basic education, or training services that were not normally provided as part of the agency's program) included JOBSTART participants only. See Auspos et al., 1989, for more details on the adaptations the sites made for JOBSTART.

[9] In order to spread average unit costs among all experimentals and to cover the full period of their involvement in the program, the participation measures captured participation that occurred at any time during the demonstration, not just within the steady-state period.

II. Accounting for Site Variations in Average Costs

The average total cost of JOBSTART's core components per experimental varied widely across the sites. (See Table E.1.) Although it fell within $4,500 to $6,500 in most sites, it ranged from less than $2,100 in CET/San Jose, EGOS in Denver, and SER/Corpus Christi to a high of about $7,500 in BSA in New York City. Several factors account for this diversity. Most notably, the sites differed in terms of *both* the amount of experimentals' participation in JOBSTART and the unit cost of that participation. This can be seen in Table E.1, which presents information for each site on the average number of months that the experimental group participated in program activities and the average monthly cost of participation.[13] Sites where the value of both of these variables was lower than in other sites were among the least expensive JOBSTART programs. For example, this combination of factors helps to explain why CET/San Jose (where experimentals participated for only 4.4 months and the average monthly cost was only $462) had the least expensive JOBSTART program.

In several sites, higher unit costs were somewhat offset by shorter participation, yielding lower total average costs than were observed in some other sites. For example, the average

---

[10] This approach (that is, multiplying the average cost of serving one person for one month by the average number of months that JOBSTART experimentals spent in the component) allocates costs between JOBSTART and non-JOBSTART participants on the basis of their respective lengths of stay in the activity.

[11] This approach assumes that there was no difference in the average cost of serving JOBSTART and non-JOBSTART participants who actually began the component. The costs were considered fixed, whether or not the students stayed until completion. No data were available for comparing the lengths of stay of JOBSTART and non-JOBSTART participants.

[12] See Chapter 2 for a discussion of the sample used in this report. In general, the experimentals in this sample, on average, had slightly more months with any hours of participation than did all experimentals, and thus may have been slightly more expensive to serve than those not in this sample.

[13] The average monthly cost for each site – created to facilitate comparisons across sites – was calculated by dividing the average total cost (for experimentals) by the average number of months active in JOBSTART (for experimentals). Although this calculation assumes that the average total cost in all sites was variable, as noted earlier, the costs of fixed-cycle activities were actually calculated on a fixed-cycle basis.

TABLE E.1

AVERAGE MONTHLY AND TOTAL COSTS PER EXPERIMENTAL AND PER PARTICIPANT, BY SITE

Site	Percent of Experimentals with Any Hours of Participation	Average Number of Months Active in JOBSTART[a]		Average Cost per Month Active in JOBSTART	Average Total Cost for Core Components	
		Experimentals	Participants		Experimentals	Participants[b]
Concurrent						
Atlanta Job Corps	84.80%	4.94	5.82	$845	$4,173	$4,921
CET/San Jose	64.00	4.40[c]	6.88[c]	462	2,034	3,178
Chicago Commons	91.90	4.32	4.71	1,499	6,477	7,048
Connelley (Pittsburgh)	98.90	9.16	9.26	566	5,185	5,243
East LA Skills Center	82.40	5.04	6.12	970	4,887	5,931
EGOS (Denver)	93.55	6.86[d]	7.33[d]	303	2,076	2,219
Phoenix Job Corps	86.57	6.25	7.22	793	4,956	5,725
SER/Corpus Christi	98.30	5.03	5.12	417	2,098	2,134
Sequential/in-house						
El Centro (Dallas)	100.00	5.25	5.25	1,011	5,306	5,306
LA Job Corps	79.10	7.17	9.05	774	5,550	7,016
Sequential/brokered						
Allentown (Buffalo)	100.00	8.88	8.88	660	5,862	5,862
BSA (NYC)	75.38	4.77	6.33	1,569	7,484	9,928
CREC (Hartford)	88.89	5.60	6.30	923	5,166	5,812

SOURCE: MDRC calculations from site and MDRC participation, fiscal, and administrative data.

NOTES: Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate. "Participants" are the subset of experimentals who were active for at least one hour in any JOBSTART component within 24 months of random assignment.

All costs are in 1986 dollars.

[a] Unless otherwise stated, the number of months active in JOBSTART is defined as the number of months with hours in any JOBSTART component.

[b] These estimates were obtained by dividing the average total cost per experimental by the percentage of experimentals with any hours of participation.

[c] For consistency with the definition of unit costs in this site, the number of months active in JOBSTART is measured from the month of random assignment to the last month with hours in any component.

[d] For consistency with the definition of unit costs in this site, the number of months active in JOBSTART is measured from the first month with hours in any JOBSTART component to the last month with hours in any component.

monthly cost of JOBSTART at the Atlanta Job Corps was higher than at Connelley in Pittsburgh ($845 compared to $566). However, Atlanta's overall average cost was lower ($4,173 compared to $5,185) because its experimentals were active in the program for less time (4.94 months compared to 9.16 months).

The wide variation in average monthly costs across the sites (ranging from $303 in EGOS in Denver to $1,569 in BSA in New York City) has a number of sources. One is enrollment levels. For example, if the number of participants "on-board" a program in a typical month is high relative to the number of program instructors, the total monthly instructional costs (and the corresponding overhead expenditures) will be spread over many people, lowering the average unit cost per participant. This factor helps to account for the relatively low monthly cost of JOBSTART at EGOS in Denver, a large public vocational school with more than 15,000 students. In contrast, at BSA in New York City, high monthly costs were partly the result of its having enrolled only about half the number of students the school had the capacity to serve at any one time. Staffing decisions can also affect costs. For example, Chicago Commons assigned two instructors to all training classes, an unusual practice among the JOBSTART sites, and this raised its average monthly cost per participant. Differences in wage scales further explain some of the variation in monthly costs. As an illustration, the average hourly wage paid to instructors at SER/Corpus Christi was about half the hourly rate received by teachers at the East Los Angeles Skills Center.

Differences in the scope of activities and services across the sites also account for differences in average monthly costs. For instance, as will be seen below, the three least expensive sites had no life skills or work-readiness instruction, and one of them (CET/San Jose) spent little on support service payments and basic education as a separate activity. Differences in overhead costs, such as those for rent and administration, also varied across the JOBSTART sites.

Table E.1 shows that in some sites a substantial proportion of experimentals left the program after random assignment and so never entered any program component. At the East Los Angeles Skills Center, for example, only 82 percent of the experimental group ever received JOBSTART services. One consequence of such attrition is that a site's average cost *per person* *actually served* by the program is higher than its average cost *per experimental*.[14] At the Los Angeles Job Corps, it was 26 percent higher ($7,016 compared to $5,550). Although the average cost per experimental would be the appropriate number to include in a benefit-cost analysis for the JOBSTART evaluation, the average cost per participant may be a better guide for administrators interested in the implications for an agency's budget of operating a JOBSTART program.

III. Component Costs

This section discusses how the costs of the individual JOBSTART components contributed to the total average cost at each site and further illustrates the sources of variation in those total costs across the sites.

A. Definitions of Components

For purposes of the cost analysis, JOBSTART functions were divided into eight main components. The category of *recruitment, intake, and orientation* was defined to encompass sites' efforts to attract and enroll individuals into the JOBSTART program and to prepare them, through special presentations or workshops, for attending the agency's regular education and training classes. This process involved screening applicants to determine whether they met all JOBSTART as well as JTPA or Job Corps eligibility criteria.[15] Random assignment, special data collection, and the additional efforts devoted to recruiting and processing extra individuals to allow the creation of a control group also occurred during the recruitment and intake stages. As previously mentioned, these latter activities were defined as research-only costs, and hence they were not counted in the average cost of this component.

Following orientation, experimentals in all sites were scheduled to attend *basic education* classes. *Occupational skills training* classes were offered concurrently or following the completion of basic education. The costs of these two components were estimated separately, although in some sites the line between them was not sharp.[16]

---

[14] The average cost per participant was calculated by dividing the average cost per experimental by the percentage of experimentals with any hours of participation.

[15] The costs included here for JTPA eligibility determination only cover a site's efforts to help applicants identify and collect the necessary documents and complete the required paperwork as part of the application process. It generally does not include the time JTPA staff spent reviewing those documents and approving the applications.

Several sites also enriched their programs by offering *work-readiness classes or life skills workshops* that covered topics such as work habits, health, and financial management. In addition, the Job Corps sites offered avocational classes in drivers education, sewing, and physical education. These were counted as part of the work-readiness/life skills component for the cost analysis.

Job placement was defined to include instruction in job-seeking techniques as well as direct placement efforts. *Coordination and counseling* include staff efforts to monitor participants' attendance and progress in JOBSTART activities and to counsel them on an as-needed basis. In a number of sites where JOBSTART was operated alongside other programs, a special counselor was designated to perform this function exclusively for the experimental group.

Support services are defined as the special expenditures intended to help motivate participants to attend program activities regularly, or to help offset some of the potential barriers to attendance. The particular types of support services that were available varied across sites, but included payments for child care, transportation, needs-based payments, food, and attendance and achievement awards.

Medical and dental services were an additional component offered in the three Job Corps sites through an on-site clinic. To a much lesser extent, such services were also offered at EGOS in Denver through a formal agreement with an outside agency to which staff routinely referred participants.[17] These services are not considered to be part of the core JOBSTART model, however. Consequently, their costs have not been included in the total average costs reported above (although they have been estimated).

It should be noted that the information used in allocating total site costs across components (such as the proportion of staff time spent on recruitment and intake versus counseling and coordination) was often imprecise. As a result, some component cost estimates are much less certain than others. However, this does not affect the estimate of any site's average total cost.

---

[16] Data limitations have precluded perfect consistency across all sites in the definition of each component. Especially problematic is the distinction between basic education and skills training at CET/San Jose and Chicago Commons. To a large extent, basic education instruction in those two sites was integrated with occupational skills training. However, those sites also operated separate remedial education classes. The cost analysis counts only participation in those remediation classes as basic education. This definition of basic education is consistent with that used in the calculation of education hours, as reported in Chapter 3.

[17] Some of the other JOBSTART sites also referred participants to such services at outside agencies, but on a much less formal basis. In these sites, the costs incurred by those agencies were not estimated.

B. Variations in the Cost of JOBSTART Components

Table E.2 shows the estimated average cost per experimental of each JOBSTART component for each site. In addition, for the four sites where medical and dental services were provided, the table shows how the total average cost changes when expenditures for these services are counted. In the three Job Corps sites, these amounted to fairly sizable expenditures – $564 per experimental at Los Angeles, $690 at Atlanta, and $357 at Phoenix. In addition, EGOS in Denver, which, through routine referrals, provided eyeglasses and dental examinations, spent $24 per experimental on medical expenses.

Recruitment, intake, and orientation activities accounted for between 9 and 13 percent of the total average cost of the core JOBSTART components in most of the sites, but reached as high as 19 percent at the East Los Angeles Skills Center. In absolute value, BSA in New York City spent the most on these upfront efforts ($1,313), while SER/Corpus Christi spent the least ($227 per experimental).

Basic education, skills training, and coordination and counseling were usually among the most expensive components to operate across the sites. When taken together, they accounted for no less than 44 percent (the estimate for the Atlanta Job Corps) of the total average cost of the core JOBSTART components, and they reached as high as 86 percent (the estimate for SER/Corpus Christi). In most of the sites, this combination of functions accounted for at least two-thirds of the total average cost.

Particularly notable are the high costs of basic education at the three sequential/brokered sites. At BSA in New York City and CREC in Hartford, basic education alone accounted for half the total cost ($3,836 and $2,634 per experimental, respectively). The cost of basic education was also high at Chicago Commons ($1,400), which added a separate education class specifically for JOBSTART participants, and at El Centro in Dallas ($1,301) and the East Los Angeles Skills Center ($1,114). Overall, the percentage of total average costs devoted strictly to basic education ranged from 4 percent at CET/San Jose to 51 percent at BSA and CREC. Education costs were especially low at CET/San Jose ($88 per experimental) in part because most of the hours that experimentals spent in that site were spent in training classes, which, it should be recalled, also

TABLE E.2

AVERAGE JOBSTART OPERATING COSTS PER EXPERIMENTAL, BY SITE

	Concurrent								Sequential/ In-House		Sequential/ Brokered		
Component	Atlanta Job Corps	CET/ San Jose	Chicago Commons	Connelley (Pittsburgh)	East LA Skills Center	EGOS (Denver)	Phoenix Job Corps	SER/ Corpus Christi	El Centro (Dallas)	LA Job Corps	Allentown (Buffalo)	BSA (NYC)	CREC (Hartford)
Recruitment, intake, and orientation	$602	$245	$327	$445	$923	$245	$645	$227	$568	$586	$328	$1,313	$689
Basic education	529	88	1,400	644	1,114	384	939	632	1,301	648	1,147	3,836	2,634
Occupational skills training	283	1,031	2,931	793	1,531	297	1,446	533	1,175	1,478	529	453	332
Work-readiness or life skills	839	n/a	35	283	n/a	n/a	343	n/a	392	442	1,438	920	n/a
Job development and placement assistance	92	308	262	334	36	19	188	73	639	302	628	n/a	n/a
Counseling and program coordination	1,031	301	1,159	2,239	1,196	664	498	633	719	705	1,058	757	1,279
Support services[a]	797	61	363	447	87	467	897	593[b]	512	1,389	734	205	232
Subtotal for core JOBSTART components	4,173	2,034	6,477	5,185	4,887	2,076	4,956	2,098	5,306	5,550	5,862	7,484	5,166
Medical/dental[c]	690	n/a	n/a	n/a	n/a	24	357	n/a	n/a	564	n/a	n/a	n/a
Total	4,863	2,034	6,477	5,185	4,887	2,100	5,313	2,098	5,306	6,114	5,862	7,484	5,166

SOURCE: MDRC calculations from site and MDRC participation, fiscal, and administrative data.

NOTES: Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate.

All costs are in 1986 dollars.

[a] Includes such costs as needs-based and incentive payments; transportation, child care, and clothing allowances; and food.

[b] Because of data limitations, the support services cost for JOBSTART and non-JOBSTART participants in this site could not be separated from other expenditures in the general overhead rate used in estimating the cost of the other program components. Thus, the per-experimental cost of each component includes the cost for support services. To avoid double-counting this expenditure in the average total cost per experimental, the $593 estimated value of support services, which was calculated from individual-level data available only for JOBSTART youths, is not included in the sum of component costs.

[c] These services were routinely available in and measured for the Job Corps sites and EGOS only.

BEST COPY AVAILABLE

included some work on basic education skills. (Basic education that occurred in the context of occupational skills training was counted as skills training.)

The resources spent on skills training also varied widely across the sites. Not surprisingly, as a proportion of total average costs, expenditures for this component were lowest at the sequential/brokered sites (accounting for less than 10 percent of those costs), where only about one-quarter of the experimentals made the transition to a training activity. In the other sites, this component accounted for between 7 percent and 51 percent of total costs. In absolute value, it was most expensive at Chicago Commons ($2,931), where the training classes were small and operated on a fixed-cycle basis (dropouts within a cycle were usually not replaced with other students). The classes also involved fairly technical instruction and used significant amounts of purchased and donated supplies.

Across all sites, the per-experimental cost of coordination and counseling ranged from $301 at CET/San Jose to $2,239 at Connelley in Pittsburgh, where it accounted for an unusually high 43 percent of the total average cost per experimental. In contrast, job search assistance and placement in most sites accounted for no more than 6 percent of total average costs, although it ranged from $19 per experimental at EGOS in Denver to $639 at El Centro in Dallas. In most sites, this tended to be a relatively inexpensive component because relatively few staff members were usually assigned to instruct or assist many different participants. (In two of the three sequential/brokered sites, job search assistance was not offered by the sponsoring agencies but, instead, was a function expected to be performed by the outside training vendors.)

The sites also varied widely in their expenditures on work-readiness and life skills instruction. Indeed, some sites did not offer these activities at all, while others gave them considerable emphasis. Allentown in Buffalo, for example, spent more per experimental on life skills than it did on education ($1,438, or 25 percent of total average costs).

Expenditures on support service costs ranged from $61 per experimental at CET/San Jose to $1,389 (or 25 percent of the total average cost) at the Los Angeles Job Corps. The other Job Corps sites, as well as SER/Corpus Christi, also devoted a relatively high proportion of resources to these services – about one-fifth to more than one-fourth of total average costs.

Table E.3 shows the breakdown of support services costs for child care, transportation, food, needs-based payments, and other purposes (which included incentives for attendance and

TABLE E.3

AVERAGE JOBSTART SUPPORT SERVICES AND PARTICIPANT PAYMENT COSTS PER EXPERIMENTAL, BY SITE

	Concurrent								Sequential/ In-House		Sequential/ Brokered		
Support Service	Atlanta Job Corps	CET/ San Jose	Chicago Commons	Connelley (Pittsburgh)	East LA Skills Center	EGOS (Denver)	Phoenix Job Corps	SER/ Corpus Christi	El Centro (Dallas)	LA Job Corps	Allentown (Buffalo)	BSA (NYC)	CREC (Hartford)
Child care	n/a	n/a	n/a	$107	$21	$339	n/a	$86	n/a	n/a	$276	$45	$27
Transportation	n/a	n/a	n/a	n/a	12	121	n/a	n/a	$161	$206	86	107	168
Food	$153	n/a	n/a	n/a	17	7	$221	n/a	n/a	259	n/a	n/a	n/a
Needs-based and incentive payments, clothing allowances, and miscellaneous	644	$61	$363	340	37	0	676	507	351	924	372	53	37
Total	797	61	363	447	87	467	897	593	512	1,389	734	205	232

SOURCE: MDRC calculations from site and MDRC participation, fiscal, and administrative data.

NOTES: Estimates in this table used data for all experimentals for whom there were 24 months of follow-up survey data, including those who were assigned to JOBSTART but did not participate.
All costs are in 1986 dollars.

achievement). Almost all the sites offered needs-based payments,[18] while about half of them paid for child care and transportation. Food costs were substantial in the three Job Corps sites (ranging from $153 to $259 per experimental), where the on-site cafeterias offered regular meals to both residential and nonresidential participants.

---

[18] All Job Corps participants (including those in JOBSTART) received a $40 monthly living allowance, which increased to $60 after they were active for 61 ("good") days, and increased again to $80 after they were active for 181 days. After that, participants were eligible to receive a merit pay level of $90 to $100 per month. In addition, upon termination from the Job Corps, those who remained in the program for a specified length of time received a separate "readjustment allowance" for each month of participation. This allowance ranged from $75 to $100 per month depending on the number of days they were active. Participants could have a portion of this allowance sent to a dependent family member. If they made that choice, the Job Corps contributed an equal amount to the family member.

APPENDIX F

SUPPLEMENTAL TABLES TO CHAPTERS 4 AND 5

TABLE F.1

IMPACTS ON MONTHLY RECEIPT OF EDUCATION OR TRAINING

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Received education or training				
Month 1	68.6%	5.4%	63.1***	0.000
Month 2	82.6	8.0	74.6***	0.000
Month 3	75.4	10.0	65.4***	0.000
Month 4	67.5	11.1	56.3***	0.000
Month 5	60.7	11.7	49.0***	0.000
Month 6	52.4	12.1	40.4***	0.000
Month 7	43.5	13.5	30.0***	0.000
Month 8	35.3	12.7	22.6***	0.000
Month 9	32.0	13.8	18.2***	0.000
Month 10	28.6	14.6	14.0***	0.000
Month 11	24.5	14.6	9.9***	0.000
Month 12	19.3	12.5	6.8***	0.000
Month 13	21.3	15.6	5.7***	0.001
Month 14	17.2	12.0	5.2***	0.001
Month 15	16.6	11.1	5.6***	0.000
Month 16	15.5	10.9	4.6***	0.003
Month 17	14.8	11.3	3.6**	0.022
Month 18	13.8	11.1	2.7*	0.075
Month 19	13.2	11.1	2.0	0.183
Month 20	12.1	12.2	-0.1	0.968
Month 21	12.3	12.6	-0.2	0.881
Month 22	11.7	12.8	-1.1	0.473
Month 23	11.3	11.3	0.0	0.982
Month 24	11.2	10.3	0.8	0.559
Sample size	949	890		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE F.2

IMPACTS ON MONTHLY AVERAGE HOURS OF EDUCATION OR TRAINING RECEIVED

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p^a
Hours of education or training received				
Month 1	29.44	2.51	26.93***	0.000
Month 2	69.84	6.41	63.43***	0.000
Month 3	63.16	8.57	54.59***	0.000
Month 4	52.57	10.41	42.16***	0.000
Month 5	46.26	11.06	35.20***	0.000
Month 6	39.43	11.99	27.44***	0.000
Month 7	31.59	12.91	18.68***	0.000
Month 8	27.02	12.92	14.10***	0.000
Month 9	24.79	14.41	10.38***	0.000
Month 10	21.97	14.15	7.82***	0.000
Month 11	17.35	12.91	4.43**	0.012
Month 12	15.60	11.79	3.81**	0.031
Month 13	16.60	10.78	5.82***	0.001
Month 14	18.66	10.50	8.16***	0.000
Month 15	18.42	10.00	8.41***	0.000
Month 16	17.86	9.71	8.15***	0.000
Month 17	15.72	9.77	5.95***	0.001
Month 18	15.40	9.38	6.01***	0.001
Month 19	14.47	9.63	4.84***	0.007
Month 20	13.56	9.83	3.73**	0.036
Month 21	13.36	10.47	2.89	0.111
Month 22	12.49	10.54	1.95	0.281
Month 23	12.39	10.02	2.37	0.187
Month 24	11.21	9.45	1.76	0.290
Sample size	949	890		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

For experimentals, "hours of education or training" include JOBSTART hours from MIS data and non-JOBSTART hours from survey data.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE F.3

IMPACTS ON MONTHLY RECEIPT OF BASIC EDUCATION

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Received basic education				
Month 1	47.8%	3.1%	44.7***	0.000
Month 2	78.9	3.7	75.2***	0.000
Month 3	70.3	4.9	65.5***	0.000
Month 4	59.1	5.5	53.6***	0.000
Month 5	50.3	6.5	43.8***	0.000
Month 6	36.1	7.3	28.8***	0.000
Month 7	29.0	7.9	21.1***	0.000
Month 8	23.0	8.0	15.0***	0.000
Month 9	18.7	8.6	10.1***	0.000
Month 10	15.5	8.6	6.9***	0.000
Month 11	14.4	8.3	6.1***	0.000
Month 12	11.3	7.2	4.1***	0.002
Month 13	13.9	10.3	3.6**	0.017
Month 14	12.3	8.4	3.9***	0.006
Month 15	11.5	7.1	4.4***	0.001
Month 16	10.8	6.8	4.0***	0.003
Month 17	10.2	7.1	3.1**	0.017
Month 18	9.0	7.2	1.8	0.155
Month 19	8.8	7.1	1.6	0.193
Month 20	7.8	7.1	0.7	0.572
Month 21	7.7	7.8	-0.1	0.952
Month 22	7.4	8.1	-0.7	0.570
Month 23	7.6	7.0	0.6	0.652
Month 24	7.4	6.3	1.1	0.352
Sample size	949	890		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Basic education" includes JOBSTART and non-JOBSTART education activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE F.4

IMPACTS ON MONTHLY RECEIPT OF TRAINING

Outcome and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Received training				
Month 1	30.7%	3.8%	26.9***	0.000
Month 2	47.4	6.0	41.4***	0.000
Month 3	47.8	7.6	40.2***	0.000
Month 4	45.7	8.8	37.0***	0.000
Month 5	44.2	9.2	35.1***	0.000
Month 6	41.7	9.3	32.4***	0.000
Month 7	34.6	10.3	24.3***	0.000
Month 8	28.8	9.8	19.0***	0.000
Month 9	26.4	10.7	15.7***	0.000
Month 10	24.1	11.1	13.0***	0.000
Month 11	18.3	11.5	6.8***	0.000
Month 12	15.5	10.0	5.5***	0.000
Month 13	16.8	12.2	4.6***	0.005
Month 14	12.6	8.1	4.5***	0.002
Month 15	12.1	7.8	4.4***	0.002
Month 16	11.1	8.0	3.1**	0.020
Month 17	10.7	8.3	2.4*	0.076
Month 18	10.6	8.3	2.3*	0.089
Month 19	9.8	8.2	1.6	0.219
Month 20	9.0	9.1	0.0	0.973
Month 21	9.1	9.3	-0.1	0.928
Month 22	8.6	8.9	-0.3	0.824
Month 23	8.5	8.0	0.4	0.734
Month 24	8.2	7.5	0.7	0.592
Sample size	949	890		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Training" includes JOBSTART and non-JOBSTART occupational skills training activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE F.5

IMPACTS ON MONTHLY EMPLOYMENT RATES, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Men				
Ever employed (%)				
Month 1	16.5	27.7	-11.2***	0.000
Month 2	21.3	32.7	-11.4***	0.000
Month 3	24.0	36.9	-12.9***	0.000
Month 4	28.3	41.6	-13.3***	0.000
Month 5	29.6	44.2	-14.5***	0.000
Month 6	33.0	44.0	-11.0***	0.001
Month 7	37.6	44.8	-7.2**	0.028
Month 8	38.8	45.7	-7.0**	0.034
Month 9	41.9	49.3	-7.3**	0.027
Month 10	47.0	52.0	-4.9	0.136
Month 11	46.0	54.8	-8.8***	0.008
Month 12	50.9	54.1	-3.2	0.335
Month 13	58.3	58.8	-0.5	0.876
Month 14	48.2	48.7	-0.6	0.868
Month 15	52.4	50.5	2.0	0.560
Month 16	52.6	51.5	1.1	0.742
Month 17	51.9	54.0	-2.2	0.513
Month 18	54.2	55.8	-1.7	0.608
Month 19	54.1	57.1	-3.0	0.368
Month 20	56.7	57.0	-0.3	0.932
Month 21	57.1	57.5	-0.4	0.913
Month 22	59.8	60.5	-0.8	0.819
Month 23	58.0	60.7	-2.7	0.423
Month 24	59.7	62.9	-3.2	0.339
Sample size	438	433		
Women living with own child(ren)				
Ever employed (%)				
Month 1	6.1	8.4	-2.3	0.340
Month 2	5.9	11.7	-5.8**	0.026
Month 3	7.9	13.4	-5.5*	0.054
Month 4	9.6	15.4	-5.8*	0.058
Month 5	12.2	16.0	-3.8	0.232
Month 6	15.6	17.0	-1.4	0.678
Month 7	18.2	19.0	-0.8	0.827
Month 8	22.2	19.9	2.3	0.533
Month 9	24.6	22.4	2.1	0.574
Month 10	26.1	23.0	3.2	0.423
Month 11	24.9	22.9	2.0	0.609
Month 12	25.6	23.5	2.0	0.611
Month 13	30.8	23.5	7.4*	0.073
Month 14	24.8	18.4	6.3*	0.096
Month 15	24.7	22.3	2.4	0.526
Month 16	25.9	21.1	4.8	0.210
Month 17	26.1	22.9	3.2	0.414
Month 18	26.2	23.3	2.9	0.460
Month 19	26.9	24.3	2.6	0.525
Month 20	25.6	24.8	0.8	0.846
Month 21	23.5	25.7	-2.2	0.583
Month 22	23.1	25.8	-2.7	0.498
Month 23	23.3	25.1	-1.8	0.657
Month 24	23.0	24.1	-1.1	0.783
Sample size	250	234		

(continued)

TABLE F.5 (continued)

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Women not living with own child(ren), including those who did not have any				
Ever employed (%)				
Month 1	12.8	20.0	-7.3**	0.031
Month 2	17.0	23.6	-6.7*	0.068
Month 3	17.5	24.4	-6.9*	0.058
Month 4	17.6	27.0	-9.4**	0.011
Month 5	22.3	32.1	-9.8**	0.014
Month 6	24.3	34.4	-10.1**	0.014
Month 7	28.8	36.3	-7.5*	0.080
Month 8	30.4	38.9	-8.5*	0.051
Month 9	33.7	34.6	-0.9	0.834
Month 10	35.0	34.4	0.6	0.883
Month 11	37.1	36.8	0.3	0.940
Month 12	38.5	39.7	-1.1	0.794
Month 13	45.2	43.5	1.8	0.693
Month 14	37.4	32.1	5.3	0.211
Month 15	38.1	35.7	2.4	0.582
Month 16	37.7	38.4	-0.7	0.878
Month 17	36.9	39.8	-2.9	0.506
Month 18	37.4	40.1	-2.7	0.543
Month 19	37.0	40.1	-3.1	0.498
Month 20	36.4	37.6	-1.2	0.794
Month 21	44.7	36.9	7.8*	0.086
Month 22	45.3	33.6	11.7***	0.010
Month 23	45.6	39.0	6.5	0.151
Month 24	44.1	40.3	3.8	0.406
Sample size	261	223		

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from split-file linear analysis of covariance procedures controlling for up to 29 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE F.6

IMPACTS ON MONTHLY RATES OF POSITIVE ACTIVITY, BY GENDER AND PARENTAL STATUS

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p^a
Men				
Employed or in education or training (%)				
Month 1	75.7	29.4	46.3***	0.000
Month 2	89.2	35.5	53.7***	0.000
Month 3	85.5	40.0	45.5***	0.000
Month 4	80.2	45.0	35.2***	0.000
Month 5	76.5	49.4	27.2***	0.000
Month 6	71.9	50.1	21.7***	0.000
Month 7	67.6	51.2	16.4***	0.000
Month 8	61.9	52.0	9.9***	0.003
Month 9	64.2	55.4	8.7***	0.009
Month 10	65.4	58.6	6.8**	0.039
Month 11	60.7	61.7	-1.0	0.757
Month 12	60.3	59.1	1.3	0.702
Month 13	68.6	66.0	2.6	0.408
Month 14	59.4	54.9	4.5	0.180
Month 15	61.8	55.3	6.5**	0.048
Month 16	62.6	55.6	7.1**	0.029
Month 17	62.3	58.5	3.8	0.236
Month 18	63.1	59.5	3.5	0.279
Month 19	63.4	61.7	1.7	0.592
Month 20	65.0	61.5	3.5	0.279
Month 21	65.7	61.7	4.0	0.210
Month 22	67.1	66.3	0.8	0.812
Month 23	64.6	65.8	-1.2	0.703
Month 24	64.6	67.3	-2.7	0.396
Sample size	438	433		
Women living with own child(ren)				
Employed or in education or training (%)				
Month 1	71.6	12.8	58.8***	0.000
Month 2	84.4	16.2	68.2***	0.000
Month 3	75.0	21.2	53.8***	0.000
Month 4	71.1	24.4	46.7***	0.000
Month 5	63.7	25.6	38.1***	0.000
Month 6	59.7	26.8	32.8***	0.000
Month 7	53.9	30.0	24.0***	0.000
Month 8	53.4	31.0	22.5***	0.000
Month 9	51.5	37.3	14.2***	0.002
Month 10	50.3	36.8	13.5***	0.003
Month 11	48.6	36.9	11.7***	0.010
Month 12	44.7	35.2	9.5**	0.036
Month 13	48.6	36.1	12.5***	0.007
Month 14	39.0	30.6	8.4*	0.060
Month 15	39.0	32.2	6.8	0.130
Month 16	40.0	30.8	9.2**	0.040
Month 17	39.2	32.0	7.2	0.113
Month 18	40.0	30.8	9.2**	0.040
Month 19	39.2	31.7	7.5*	0.094
Month 20	35.6	35.0	0.6	0.889
Month 21	33.8	36.5	-2.7	0.545
Month 22	33.0	38.2	-5.2	0.236
Month 23	33.3	36.6	-3.3	0.459
Month 24	34.0	35.0	-1.0	0.813
Sample size	250	234		

(continued)

TABLE F.6 (continued)

Subgroup, Outcome, and Follow-Up Period	Experimentals	Controls	Difference	p[a]
Women not living with own child(ren), including those who did not have any				
Employed or in education or training (%)				
Month 1	70.5	28.7	41.9***	0.000
Month 2	84.8	36.2	48.6***	0.000
Month 3	81.2	38.6	42.7***	0.000
Month 4	76.1	42.3	33.8***	0.000
Month 5	75.5	45.3	30.2***	0.000
Month 6	68.8	47.7	21.1***	0.000
Month 7	64.1	52.4	11.7***	0.009
Month 8	60.2	53.3	7.0	0.128
Month 9	61.0	50.1	11.0**	0.017
Month 10	57.8	49.4	8.4*	0.070
Month 11	57.7	51.7	6.0	0.191
Month 12	55.4	53.6	1.8	0.689
Month 13	64.2	56.7	7.6*	0.093
Month 14	53.7	42.1	11.7***	0.010
Month 15	52.4	45.4	6.9	0.134
Month 16	50.2	48.0	2.2	0.632
Month 17	50.2	50.2	0.0	0.996
Month 18	49.9	51.1	-1.2	0.798
Month 19	48.9	53.1	-4.2	0.376
Month 20	45.8	50.9	-5.1	0.280
Month 21	54.2	48.7	5.5	0.239
Month 22	53.7	44.8	9.0*	0.057
Month 23	53.1	47.7	5.4	0.252
Month 24	51.0	50.1	0.9	0.846
Sample size	261	223		

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Positive activity" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities, as well as employment.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from linear analysis of covariance procedures controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: that is, p is the probability that average outcomes are different only because of random error. A two-tailed t-test was applied to each difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

APPENDIX G

SUPPLEMENTAL TABLES TO CHAPTER 6

TABLE G.1

EXPERIMENTAL-CONTROL DIFFERENCE IN SERVICE RECEIPT, BY SITE

Site	Sample Size	Ever Received Any Education or Training, Months 1-24		Difference	p[a]
		Experimentals	Controls		
Concurrent					
Atlanta Job Corps	61	90.4%	47.6%	42.8***	0.000
CET/San Jose	152	71.7	24.0	47.7***	0.000
Chicago Commons	74	96.3	60.7	35.6***	0.000
Connelley (Pittsburgh)	184	99.4	45.9	53.5***	0.000
East LA Skills Center	100	93.0	57.8	35.1***	0.000
EGOS (Denver)	183	95.4	50.0	45.4***	0.000
Phoenix Job Corps	130	90.3	38.6	51.7***	0.000
SER/Corpus Christi	236	101.2	34.8	66.4***	0.000
Sequential/in-house					
El Centro (Dallas)	155	98.8	26.9	71.9***	0.000
LA Job Corps	218	85.3	43.4	41.9***	0.000
Sequential/brokered					
Allentown (Buffalo)	140	100.5	70.2	30.4***	0.000
BSA (NYC)	119	85.4	52.1	33.3***	0.000
CREC (Hartford)	87	93.2	48.3	44.8***	0.000
All sites	1,839	92.7	44.2	48.4***	0.000

SOURCE: MDRC calculations from JOBSTART enrollment form, MIS, and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

"Any education or training" includes JOBSTART and non-JOBSTART education, occupational skills training, and related activities.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). "All sites" outcomes are from a linear analysis of covariance procedure for the full sample controlling for up to 31 kinds of difference in characteristics before random assignment (see Ostle, 1975, p. 461; Cave, 1987; and Appendix Table B.4). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE G.2

IMPACTS ON YEAR-TWO EMPLOYMENT RATES AND EARNINGS FOR MEN, BY SITE

Site or Statistic	Sample Size	Experimentals	Controls	Impact	p^a
		Ever Employed, Months 13-24			
Concurrent					
Atlanta Job Corps	25	93.3%	104.0%	-10.7	0.464
CET/San Jose	77	88.5	87.5	1.0	0.905
Chicago Commons	42	96.4	88.6	7.8	0.483
Connelley (Pittsburgh)	86	89.3	91.1	-1.9	0.812
East LA Skills Center	56	81.1	90.5	-9.4	0.336
EGOS (Denver)	65	75.6	82.9	-7.3	0.416
Phoenix Job Corps	63	80.7	88.3	-7.5	0.415
SER/Corpus Christi	143	76.3	75.7	0.7	0.911
Sequential/in-house					
El Centro (Dallas)	71	92.9	79.0	13.8	0.110
LA Job Corps	88	71.3	76.9	-5.6	0.468
Sequential/brokered					
Allentown (Buffalo)	58	83.5	86.0	-2.5	0.793
BSA (NYC)	66	89.6	88.8	0.8	0.926
CREC (Hartford)	31	107.1	77.4	29.7**	0.022
P-value of F-statistic				0.459	
		Total Earnings, Months 13-24			
Concurrent					
Atlanta Job Corps	25	$5,138.56	$9,153.12	-4,014.55*	0.054
CET/San Jose	77	8,028.96	7,475.22	553.74	0.635
Chicago Commons	42	6,894.20	6,273.93	620.27	0.695
Connelley (Pittsburgh)	86	2,667.97	4,191.36	-1,523.39	0.168
East LA Skills Center	56	5,622.91	8,340.70	-2,717.79**	0.049
EGOS (Denver)	65	3,961.28	4,915.83	-954.55	0.454
Phoenix Job Corps	63	4,765.22	4,847.08	-81.86	0.950
SER/Corpus Christi	143	3,509.92	3,421.01	88.91	0.918
Sequential/in-house					
El Centro (Dallas)	71	4,714.69	5,289.66	-574.97	0.639
LA Job Corps	88	4,570.40	7,537.41	-2,967.01***	0.007
Sequential/brokered					
Allentown (Buffalo)	58	5,198.10	4,480.79	717.31	0.598
BSA (NYC)	66	8,154.38	8,880.16	-725.78	0.565
CREC (Hartford)	31	8,844.29	6,040.01	2,804.28	0.127
P-value of F-statistic				0.141	

(continued)

TABLE G.2 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE G.3

IMPACTS ON YEAR-TWO EMPLOYMENT RATES AND EARNINGS FOR WOMEN LIVING WITH THEIR OWN CHILD(REN) AT THE TIME OF RANDOM ASSIGNMENT, BY SITE

Site or Statistic	Sample Size	Experimentals	Controls	Impact	p^a
		Ever Employed, Months 13-24			
Concurrent					
Atlanta Job Corps	18	39.5%	49.1%	-9.6	0.689
CET/San Jose	10	86.1	36.4	49.7	0.125
Chicago Commons	18	60.9	46.8	14.1	0.553
Connelley (Pittsburgh)	63	53.3	31.6	21.7*	0.084
East LA Skills Center	12	53.0	4.4	48.7	0.138
EGOS (Denver)	67	46.0	37.8	8.2	0.500
Phoenix Job Corps	37	46.3	43.6	2.7	0.868
SER/Corpus Christi	53	58.1	69.7	-11.6	0.394
Sequential/in-house					
El Centro (Dallas)	51	61.7	49.4	12.3	0.393
LA Job Corps	66	54.8	44.9	9.9	0.422
Sequential/brokered					
Allentown (Buffalo)	46	56.2	62.6	-6.4	0.661
BSA (NYC)	19	44.3	58.5	-14.2	0.536
CREC (Hartford)	24	40.9	25.8	15.1	0.457
P-value of F-statistic				0.676	
		Total Earnings, Months 13-24			
Concurrent					
Atlanta Job Corps	18	$1,863.38	$1,452.36	411.02	0.813
CET/San Jose	10	3,022.81	2,686.77	336.04	0.886
Chicago Commons	18	2,002.47	2,074.73	-72.26	0.966
Connelley (Pittsburgh)	63	755.92	459.09	296.83	0.743
East LA Skills Center	12	2,521.82	305.39	2,216.44	0.349
EGOS (Denver)	67	1,903.33	1,973.14	-69.82	0.936
Phoenix Job Corps	37	1,208.87	1,969.39	-760.51	0.520
SER/Corpus Christi	53	1,575.21	2,324.46	-749.25	0.446
Sequential/in-house					
El Centro (Dallas)	51	4,349.84	1,959.85	2,389.99**	0.022
LA Job Corps	66	2,450.62	1,713.48	737.13	0.404
Sequential/brokered					
Allentown (Buffalo)	46	2,162.68	2,379.27	-216.59	0.837
BSA (NYC)	19	1,992.69	5,076.42	-3,083.73*	0.063
CREC (Hartford)	24	1,894.11	1,385.64	508.47	0.729
P-value of F-statistic				0.492	

(continued)

TABLE G.3 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a]A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

TABLE G.4

IMPACTS ON YEAR-TWO EMPLOYMENT RATES AND EARNINGS FOR WOMEN NOT LIVING WITH THEIR OWN CHILD(REN) AT THE TIME OF RANDOM ASSIGNMENT, BY SITE

Site or Statistic	Sample Size	Experimentals	Controls	Impact	p^a
		Ever Employed, Months 13-24			
Concurrent					
Atlanta Job Corps	18	77.6%	70.9%	6.7	0.768
CET/San Jose	65	79.8	71.2	8.6	0.457
Chicago Commons	14	88.4	74.3	14.2	0.588
Connelley (Pittsburgh)	35	69.0	39.9	29.1*	0.068
East LA Skills Center	32	67.8	50.1	17.7	0.294
EGOS (Denver)	51	62.7	66.8	-4.2	0.748
Phoenix Job Corps	30	58.7	86.0	-27.3	0.124
SER/Corpus Christi	40	58.9	61.3	-2.3	0.875
Sequential/in-house					
El Centro (Dallas)	33	69.8	49.5	20.2	0.236
LA Job Corps	64	73.5	64.4	9.2	0.434
Sequential/brokered					
Allentown (Buffalo)	36	72.9	59.8	13.2	0.432
BSA (NYC)	34	68.3	69.1	-0.8	0.961
CREC (Hartford)	32	66.4	95.3	-28.9*	0.086
P-value of F-statistic				0.426	
		Total Earnings, Months 13-24			
Concurrent					
Atlanta Job Corps	18	$2,212.27	$4,800.24	-2,587.98	0.203
CET/San Jose	65	6,440.66	5,824.94	615.72	0.550
Chicago Commons	14	4,829.92	1,498.89	3,331.03	0.153
Connelley (Pittsburgh)	35	2,121.38	670.34	1,451.03	0.306
East LA Skills Center	32	4,504.55	2,091.86	2,412.69	0.109
EGOS (Denver)	51	1,667.32	2,741.52	-1,074.19	0.355
Phoenix Job Corps	30	1,764.39	3,590.86	-1,826.47	0.247
SER/Corpus Christi	40	1,121.12	1,566.22	-445.09	0.738
Sequential/in-house					
El Centro (Dallas)	33	2,792.78	1,838.33	954.46	0.531
LA Job Corps	64	5,480.17	3,832.21	1,647.96	0.115
Sequential/brokered					
Allentown (Buffalo)	36	2,971.29	1,622.63	1,348.66	0.368
BSA (NYC)	34	2,975.79	3,362.20	-386.42	0.790
CREC (Hartford)	32	3,795.73	7,219.70	-3,423.97**	0.023
P-value of F-statistic				0.104	

(continued)

TABLE G.4 (continued)

SOURCE: MDRC calculations from JOBSTART enrollment form and survey data.

NOTES: Calculations for this table used data for all sample members for whom there were 24 months of follow-up survey data, including those with values of zero for outcomes and those who were assigned to JOBSTART but did not participate.

Random assignment did not always take place on the first of the month. For some sample members, the month of random assignment may be less than a month, beginning with the date of random assignment and ending on the last day of the month.

Average experimental and control group outcomes reported here are adjusted means from two-way analysis of covariance procedures controlling for 19 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research assignment and site (see Ostle, 1975, p. 454). There may be slight discrepancies in reported sums and differences of these adjusted means because of rounding.

[a] A two-tailed t-test was applied to each within-site impact. An F-test was applied to the interaction between site and experimental or control status. The p-value of the F-statistic is the probability that site impacts are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

REFERENCES

American Society for Training and Development. 1988. *Workplace Basics: The Skills Employers Want.* Alexandria, Va.: American Society for Training and Development.

Auspos, Patricia. 1987. *Launching JOBSTART: A Demonstration for Dropouts in the JTPA System.* New York: Manpower Demonstration Research Corporation.

Auspos, Patricia, et al. 1989. *Implementing JOBSTART: A Demonstration for School Dropouts in the JTPA System.* New York: Manpower Demonstration Research Corporation.

Auspos, Patricia, George Cave, and David Long. 1988. *Maine: Final Report on the Training Opportunities in the Private Sector Program.* New York: Manpower Demonstration Research Corporation.

Berlin, Gordon, and Andrew Sum. 1988. *Toward a More Perfect Union: Basic Skills, Poor Families, and Our Economic Future.* New York: The Ford Foundation.

Betsey, Charles L., et al., eds. 1985. *Youth Employment and Training Programs: The YEDPA Years.* Washington, D.C.: National Research Council, National Academy Press.

Bloom, Howard. 1984. "Accounting for No-Shows in Experimental Evaluation Designs." *Evaluation Review* 8: 225-246.

Cave, George. 1988. *Noncompliance Bias in Evaluation Research: Assignment, Participation, and Impacts.* New York: Manpower Demonstration Research Corporation.

Cave, George. 1987. "Sample Sizes for Social Experiments." In *Proceedings of the Section on Survey Research Methods of the American Statistical Association.*

Clark, Kim B., and Lawrence H. Summers. 1982. "The Dynamics of Youth Unemployment." In Richard B. Freeman and David A. Wise, eds., *The Youth Labor Market Problem: Its Nature, Causes, and Consequences.* Chicago: University of Chicago Press.

Doolittle, Fred, and Linda Traeger. 1990. *Implementing the National JTPA Study.* New York: Manpower Demonstration Research Corporation.

Ekstrom, Ruth B., et al. 1986. "Who Drops Out of High School and Why? Findings from a National Study." *Teachers College Record* 87 (3): 356-373.

Ellwood, David T. 1986. *Targeting "Would-Be" Long-Term Recipients of AFDC.* Princeton, N.J.: Mathematica Policy Research.

Ellwood, David T. 1982. "Teenage Unemployment: Permanent Scars or Temporary Blemishes?" In Richard B. Freeman and David A. Wise, eds., *The Youth Labor Market Problem: Its Nature, Causes, and Consequences.* Chicago: University of Chicago Press.

Farkas, George, et al. 1984. *Post-Program Impacts of the Youth Incentive Entitlement Pilot Projects.* New York: Manpower Demonstration Research Corporation.

Friedlander, Daniel. 1988. *Subgroup Impacts and Performance Indicators for Selected Welfare Employment Programs.* New York: Manpower Demonstration Research Corporation.

Fullerton, Howard N., Jr. 1989. "New Labor Force Projections, Spanning 1988 to 2000." *Monthly Labor Review.*

Furstenberg, Frank, Jr., J. Brooks-Gunn, and S. Phillip Morgan. 1987. *Adolescent Mothers in Later Life.* New York: Cambridge University Press.

Goldman, Barbara, et al. 1986. *Final Report on the San Diego Job Search and Work Experience Demonstration.* New York: Manpower Demonstration Research Corporation.

Gordon, Anne, and John Burghardt. 1990. *The Minority Female Single Parent Demonstration: Short-Term Economic Impacts.* New York: Rockefeller Foundation.

Gueron, Judith. 1984. *Lessons from a Job Guarantee: The Youth Entitlement Pilot Projects.* New York: Manpower Demonstration Research Corporation.

Gueron, Judith M., and Edward Pauly. 1991. *From Welfare to Work.* New York: Russell Sage Foundation.

Hahn, Andrew, and Robert Lerman. 1985. *What Works in Youth Employment Policy?* Washington, D.C.: National Planning Association.

Job Training Partnership Act Advisory Panel. 1989. *Working Capital: JTPA Investments for the '90s.* Washington, D.C.: U.S. Department of Labor.

Johnson, William, and Arnold Packer. 1987. *Workforce 2000: American Work and Workers in the 21st Century.* Indianapolis: Hudson Institute.

Kelly, Jodie Sue. 1987. *JTPA Recruitment Made (Much) Easier.* Washington, D.C.: National Association of Counties.

Layard, R., and P. Psacharopoulos. 1974. "The Screening Hypothesis and the Returns to Education." *Journal of Political Economy* 82: 985-998.

Mallar, Charles, et al. 1982. *Evaluation of the Economic Impact of the Job Corps Program: Third Follow-up Report.* Princeton, N.J.: Mathematica Policy Research.

Mallar, Charles, et al. 1980. *Evaluation of the Economic Impact of the Job Corps Program: Second Follow-up Report.* Princeton, N.J.: Mathematica Policy Research.

Mallar, Charles, et al. 1978. *Evaluation of the Economic Impact of the Job Corps Program: First Follow-up Report.* Princeton, N.J.: Mathematica Policy Research.

Manpower Demonstration Research Corporation. 1988. "Report on the Conference on Youth Employment Initiatives, October 25-27, 1987." New York: Manpower Demonstration Research Corporation.

Manpower Demonstration Research Corporation. 1985. *Guidelines for JOBSTART Components.* A series of individual publications for the separate program components. New York: Manpower Demonstration Research Corporation.

Manpower Demonstration Research Corporation, Board of Directors. 1980. *Summary and Findings of the National Supported Work Demonstration.* Cambridge, Mass.: Ballinger.

Mathematica Policy Research. 1988. *The Minority Female Single Parent Demonstration: Process Analysis of Program Operations.* Princeton, N.J.: Mathematica Policy Research.

Mathematica Policy Research. 1985. *Job Corps Process Analysis: Final Report.* Princeton, N.J.: Mathematica Policy Research.

Maynard, Rebecca. 1980. *The Impact of Supported Work on Young School Dropouts.* New York: Manpower Demonstration Research Corporation.

National Academy of Sciences, National Academy of Engineering, Institute of Medicine. 1984. *High School and the Changing Workplace: The Employers' View.* Report of the Panel on Secondary School Education for the Changing Workplace. Washington, D.C.: National Academy Press.

National Association of Manufacturers. 1982. *America's Human Resources.* Washington, D.C.: Perspectives on National Issues.

Ostle, Bernard. 1975. *Statistics in Research*, 3rd ed. Ames: Iowa State University Press.

Public/Private Ventures. 1990. *The Practitioner's View: New Challenges in Serving High-Risk Youth.* Philadelphia: Public/Private Ventures.

Public/Private Ventures. 1988. *Youth Motivation: At-Risk Youth Talk to Program Planners.* Philadelphia: Public/Private Ventures.

Public/Private Ventures. 1983. *Longer-Term Impacts of Pre-Employment Services on the Employment and Earnings of Disadvantaged Youth.* Philadelphia: Public/Private Ventures.

Rees, Albert. 1986. "An Essay on Youth Joblessness." *Journal of Economic Literature* 24 (2): 613-628.

Richardson, Phillip, and John Burghardt. October 1985. *Job Corps Process Analysis: Final Report*. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, Office of Strategic Planning and Policy Development.

70001 Training and Employment Institute. 1988. *Final Report of the Disconnected Youth Demonstration Project*. Washington, D.C.: 70001 Training and Employment Institute.

Sticht, Thomas. 1987. *Cast-Off Youth: Policy and Training Methods from the Military Experience*. Westport, Conn.: Praeger.

U.S. Congress, Congressional Budget Office. 1982. *Improving Youth Employment Prospects: Issues and Options*. Washington, D.C.: U.S. Government Printing Office.

U.S. Department of Education, National Center for Education Statistics. 1988. *Education Indicators*. Washington, D.C.: U.S. Department of Education.

U.S. Department of Labor, Bureau of Labor Statistics. January 1991. *Employment and Earnings*. Washington, D.C.: U.S. Government Printing Office.

U.S. Department of Labor, Division of Performance Management and Evaluation. 1988. *JTPA Title IIA Participants Who Were School Dropouts at Program Application: Program Year 1986*. Job Training Quarterly Survey Special Paper no. 6. Washington, D.C.: U.S. Government Printing Office.

U.S. Department of Labor. 1988. *Summary of the JTQS Data for JTPA Title IIA and III Entitlements and Terminations*. Washington, D.C.: U.S. Department of Labor.

U.S. Department of Labor, Employment and Training Administration. 1987. *Job Corps in Brief: Program Year 1986*. Washington, D.C.: U.S. Government Printing Office.

U.S. Department of Labor. 1977. *Dictionary of Occupational Titles*, 4th ed. Washington, D.C.: U.S. Government Printing Office.

U.S. General Accounting Office. September 29, 1988. "Job Training Partnership Act: Participants, Services, and Outcomes." Testimony given before the Committee on Education and Labor, U.S. House of Representatives.

William T. Grant Commission on Work, Family and Citizenship. 1988. *The Forgotten Half: Pathways to Success for America's Youth and Young Families*. Washington, D.C.: William T. Grant Commission on Work, Family and Citizenship.

SELECTED MDRC PUBLICATIONS ON YOUTH PROJECTS

Book

Gueron, Judith M., and Edward Pauly. *From Welfare to Work* (Russell Sage Foundation). 1991.

Monographs

Gueron, Judith M. *Lessons from a Job Guarantee: The Youth Incentive Entitlement Pilot Projects.* 1984.

Polit, Denise, Janet Quint, and James Riccio. *The Challenge of Serving Teenage Mothers: Lessons from Project Redirection.* 1988.

Reports

Ohio's Learning, Earning, and Parenting (LEAP) Program

Bloom, Dan, Hilary Kopp, David Long, and Denise Polit. *LEAP: Implementing a Welfare Initiative to Improve School Attendance Among Teenage Parents.* 1991.

The New Chance Demonstration

Kelly, Jodie Sue, and Max Elsman. *How to Recruit Young AFDC Mothers to New Chance.* 1989.

Manpower Demonstration Research Corporation. *New Chance: Operating Guidelines for the Sites in the Demonstration.* 1989.

Quint, Janet, Barbara L. Fink, and Sharon Rowser. *New Chance: Implementing a Comprehensive Program for Disadvantaged Young Mothers and Their Children.* 1991.

Quint, Janet, and Cynthia Guy. *New Chance: Lessons from the Pilot Phase.* 1989.

Project Redirection

Branch, Alvia, and Janet Quint. *Project Redirection: Interim Report on Program Implementation.* 1981.

Branch, Alvia, James Riccio, and Janet Quint. *Building Self-Sufficiency in Pregnant and Parenting Teens: The Implementation of Project Redirection.* 1984.

Levy, Sydelle Brooks. *Choices and Life Circumstances: An Ethnographic Study of Project Redirection Teens.* 1983.

Polit, Denise. *Needs and Characteristics of Pregnant and Parenting Teens: The Baseline Report for Project Redirection.* 1982.

Polit, Denise, Janet Kahn, and David Stevens. *Final Impacts from Project Redirection: A Program for Pregnant and Parenting Teens.* 1985.

Polit, Denise, Michael Tannen, and Janet Kahn. *School, Work, and Family Planning: Interim Impacts in Project Redirection.* 1983.

Riccio, James. *Strengthening Services for Teen Mothers: The Teen Parent Collaboration.* 1985.

The Career Beginnings Demonstration

Cave, George, and Janet Quint. *Career Beginnings Impact Evaluation: Findings from a Program for Disadvantaged High School Students.* 1990.

The Youth Incentive Entitlement Pilot Projects (YIEPP)

Ball, Joseph, David Gerould, and Paul Burstein. *The Quality of Work in the Youth Entitlement Demonstration.* 1980.

Ball, Joseph, and Carl Wolfhagen. *The Participation of Private Businesses as Work Sponsors in the Youth Entitlement Demonstration.* 1981.

Diaz, William, Joseph Ball, and Carl Wolfhagen. *Linking School and Work for Disadvantaged Youths – The YIEPP Demonstration: Final Implementation Report.* 1982.

Farkas, George, Randall Olsen, Ernst W. Stromsdorfer, Linda C. Sharpe, Felicity Skidmore, D. Alton Smith, and Sally Merrill. *Post-Program Impacts of the Youth Incentive Entitlement Pilot Projects.* 1984.

Farkas, George, D. Alton Smith, Ernst W. Stromsdorfer, Gail Trask, and Robert Jerrett III. *Impacts from the Youth Incentive Entitlement Pilot Projects: Participation, Work, and Schooling Over the Full Program Period.* 1982.

Community Service Projects

Guy, Cynthia A. *Community Service Projects: A New York State Adolescent Pregnancy Initiative.* 1986.

Guy, Cynthia A., Lawrence N. Bailis, David R. Palasits, and Kay E. Sherwood. *Community Service Projects: Final Report on a New York State Pregnancy Initiative.* 1988.

Manpower Demonstration
Research Corporation

Three Park Avenue
New York, New York 10016
(212) 532-3200

1669 Bush Street
San Francisco, California 94109
(415) 441-7607

MDRC